Bund

SCIENTIFIC AMERICAN SUPPLEMENT

Copyright 1914 by Munn & Co., Inc.

VOLUME LXXVIII
NUMBER 2011

NEW YORK, JULY 18, 1914

10 CENTS A COPY
$5.00 A YEAR

A three-ton hammer.

THE MANUFACTURE OF CRUCIBLE STEEL.—[See page 40.]

The Channel Tunnel and Its Early History—II*

Plans for Direct Railway Connection Between England and France

By John Clarke Hawkshaw, M.A.

Concluded from Scientific American Supplement No. 2010, Page 19, July 11, 1914

I do not attach much value to deductions as to the probable conditions of the beds below sea-level made from observed conditions above the sea-level, for the constant movement of the land waters, from the highest levels to the points where they are discharged into the sea, must produce well-defined drainage channels underground, which need not necessarily exist at greater depths where there may be no such rapid circulation.

Still, as much has been made, especially by the French engineers, of evidence derived from observations in the zones from which water is discharged on land, I will give a few facts relating to them. In France water may be seen to flow in many places from the lower beds of chalk, where they appear in the cliffs between Escalles and St. Pot. I saw water flowing at the very junction of the lowest beds and the greensand. There are some large springs near Escalles, which on the section accompanying the French report (1877) are shown at about 60 feet above the upper greensand. The section shows Lydden Spout in England on the same horizon, but that copious and well-known spring, on the coast between Folkestone and Dover, issues at the top of the so-called "cast bed," about thirty-two feet above the upper greensand, or 46 feet above the gault. Therefore, if this evidence is worth anything it shows that, on the French side, water might be met with largely, 60 feet above the greensand, and on the English side at only 32 feet above it. Thus, if the evidence from the permeability of the strata on land proves anything, it is that a tunnel driven in the lowest beds of chalk must come very near a bed from which large springs are discharged on land, and that it would probably have to pass into this bed.

It has not only been asserted that very little or no water will be met with in the lowest beds of chalk, but also that so much will be met with in the higher beds as to make it impracticable to tunnel through them. Large numbers of wells have been sunk all over the country, and other excavations have been made at all depths and in all parts of the chalk formation, for the purpose of obtaining water, and our knowledge of the mode in which water occurs in that formation is fairly complete.

The following are, I believe, well-established facts on the subject in question:

Solid chalk absorbs a large amount of water, but it parts with it with extreme slowness, so that it cannot be said to be truly permeable. It is only from the fissures which traverse chalk formation and from cavities found along its planes of stratification that water can be obtained. At a variable depth below the surface of the land the body of the chalk is saturated, and such fissures or cavities as may occur there are full of water, not stationary, but slowly flowing from the higher ground inland toward the river valleys and seashore, where the water can find an outlet. Thus, there is a circulation of water underground just as there is above ground in other less porous formations. Moreover, as the streams and watercourses on the surface are independent of one another, so are the underground channels in the chalk.

If in excavating the chalk a fissure is cut across, a certain supply of water may be obtained; if a second fissure is cut through, a further supply may be obtained; and generally it will be found that, if water is pumped from one fissure, it does not immediately diminish the flow from adjoining fissures, and in like manner, if the flow of water from one fissure is checked, it does not necessarily increase the flow from those adjoining. In course of time lowering the water level at any fissure will affect the supply to adjoining fissures, inasmuch as all draw their supply from the same saturated mass of chalk; but as a consequence of the slowness with which chalk gives up its water, unless there is direct communication between two fissures, operations at one do not generally affect the others. Now this property of the chalk is of the greatest importance in carrying out engineering works which require excavations to be made through the water-bearing levels in it, for it enables the engineer to deal with the water in detail. Fissure after fissure may be cut through as long as the water flowing from them leaves a sufficient margin of unused pumping power; as soon as the yield of water becomes so great as to diminish that margin unduly, some of the fissures may be blocked up with temporary or permanent work. Thus it will be seen that, in tunneling through the chalk, a very large volume of water might be dealt with if it entered the work by separate channels. A very large body of water entering at one point might give trouble, and I shall accordingly select some facts from the very large number available to show that from no one area of chalk, even of considerable extent, much less from one fissure, has a supply of water been derived larger than could be easily dealt with, or which at all approaches the volumes which have been dealt with on works completed or now in progress.

At Goldstone Bottom, one of the pumping stations of the Brighton Waterworks, a greater quantity of water was obtained than at any other locality known to me when I dealt with this question in a paper read at a meeting of the British Association at Southampton in 1882. Mr. Easton, the best authority on this subject, kindly furnished me with some particulars at the time respecting these works. There were engines at Goldstone Bottom capable of raising 300,000 gallons an hour, or 5,000 gallons a minute; but it was necessary to make a length of 1,800 feet of tunnels to obtain the required supply. These were at right angles to the fissures, which there, as is so often the case, were at right angles to the shore line. At Lewes Road, another Brighton pumping station, it was necessary to drive 2,400 feet of tunnels to supply pumps capable of lifting 280,000 gallons an hour. Mr. Easton then told me, and this is the important point, that the water in those tunnels was under complete control, so that any one of them could be laid dry at any time of the year.

A work which tested to the fullest our power to tunnel through the wettest parts of the chalk was the Brighton intercepting sewer, for which Sir John Hawkshaw was the engineer. The main sewer is more than seven miles long. The outfall is placed on the coast about four miles to the east of Brighton, and throughout a distance of four and a half miles a tunnel from 9 feet to 10 feet in diameter was excavated in the upper chalk along the base of the cliff, and close to the shore. Being below high-water level, it cut through all the fissures which discharge the drainage of a large tract of inland chalk country. Large fissures were cut through, which I had frequent opportunities of inspecting, and, moreover, of seeing the permanent work successfully carried across them. The greatest quantity of water pumped at one time was 10,000 gallons a minute, or 600,000 an hour. Many other examples might be given, some of much later date, but none so striking as the above. It may be urged that, by going below the levels reached by the above works, more water would have been found; but that by no means follows. The Brighton sewer works must have intercepted most of the land drainage; by going deeper some water stored in cavities below sea-level might have been pumped. But the supply depends, not on the quantity stored, but the rate at which the fissures can deliver it, and the rate at which the chalk can supply the fissures, which we know is extremely slow.

Fissures there are, or there have been, at all depths, but as the circulation is not so rapid in the deeper ones, they are more liable to be choked by sedimentary and crystalline deposits. Deep mines are often very dry ones.

As an example of the water supply obtained from deep down in the chalk, we may take the supply drawn from the London basin. Below London we have a great thickness of both the upper and lower chalk, amounting together to more than 600 feet in places. As this chalk, so far as we know, rests in a basin of gault clay, and rises in continuous beds to form the high ground to the north and south of the London basin, it is in the best position to be saturated with water, and such was the condition before it was made use of as a source of water supply for London and its neighborhood. As the supply from the shallow wells made in the tertiary beds became exhausted, deeper wells were sunk into the chalk beneath, and water was obtained in plenty. But the supply soon ceased to be equal to the demand; the level of the water in the chalk was depressed, so that wells had to be deepened and new adits made to cut new fissures. In 1838 the quantity pumped in London was estimated at 6,000,000 gallons a day; in 1850 the quantity had increased to not more than 12,000,000 gallons a day. In the meantime the level of the water in the chalk had been depressed in some places 50 feet. Thus the larger quantity could only be obtained at a great depth, and by increasing the fall in the fissures delivering the water to the pumps. Now this chalk, from which only a limited supply could be obtained at a given depth over a large area, was connected on all sides with an unexhausted reservoir, at a much higher level, formed by the saturated masses of chalk round the London basin, and yet the water level in it was reduced by a comparatively small amount of pumping.

In the Severn tunnel we pumped 24,000,000 gallons a day, twice the quantity which, when pumped in the London basin, lowered the level of the water in the saturated chalk 50 feet. The Severn tunnel is the longest railway tunnel in England, in all four and a quarter miles in length, and it is the only railway tunnel which can claim to be submarine. It passes for two and a half miles below a tideway where there is a rise of tide of 40 feet and a depth of water of 90 feet in the deepest part of the channels. It passes for a considerable part of its length through the triassic red marl, which has many points of resemblance to chalk. The marl, which lies in horizontal beds, was much fissured, and from these fissures, as well as from the spaces between the planes of bedding, much water was discharged. For some distance at the English end of the submarine part of the tunnel there is only 35 feet to 40 feet of this open-jointed red marl above the brickwork of the tunnel. Salt water flowed freely into the work, and to show how the water channels are disconnected, salt and fresh water have in some cases flowed from adjoining fissures, and the fresh water was allowed to flow for drinking purposes through pipes built into the brickwork. The largest quantity of water and the largest spring were met with in the land portion of the tunnel. The spring referred to discharged 5,000 gallons a minute, and burst suddenly into a heading which had been driven for over 1,000 feet in millstone grit without meeting with any water. If the heading had been driven at a level of 10 feet lower, the spring would have been avoided in the heading, but would have been met with when it was enlarged to the full size of the tunnel.

From the examples I have given it will be seen that engineering works need not be stopped, even by large quantities of water, and from what we know of the chalk and its water-bearing qualities, there is nothing to show that water would be met with in such large quantities under the English Channel as to stop tunneling even in the upper chalk with flints.

As already mentioned, a tunnel from Fan Hole to Sangatte would be 20.8 miles long, and is practically as short a line as can be well obtained. It would be in a straight line, with gradients fixed beforehand, running wholly in the upper part of the lower chalk without flints, with an ample thickness of chalk above and below, and no fear of running out of it, as would be the case near the outcrop on the Folkestone route. If it were worth while, the tunnel could be made from Fan Hole for three quarters of the distance in the lowest beds of chalk by curving the line to the southward after leaving the shore, but it would be one and a half miles longer.

The submarine tunnel on the Shakespeare Cliff line would be about three miles longer.

On the Fan Hole-Sangatte line we know almost certainly that we shall have a vertical thickness of some 500 feet of chalk to tunnel through, giving about 250 feet above and almost as much below the center line of the tunnel. Along the Folkestone line there cannot be more than 250 feet of chalk, and that thickness is uncertain, as the dip of the chalk and greensand from its outcrop is purely conjectural; so that neither the gradients nor the line of the tunnel can be fixed beforehand, and the making of it will be risky and tentative from beginning to end.

I hope I have shown the danger of trying to feel one's way with borings, and a serpentine tunnel might at any time lead into difficulties.

There is no difficulty of tunneling in beds even of the upper chalk saturated with water and full of open fissures on the land. Why should an engineer be afraid of tunneling in the upper beds of the lower chalk under the sea where open fissures are not so likely to be met with? Open fissures will form under the sea as on

* Paper read before the Royal Society of Arts, and published in its *Journal*.

land, if an outlet is provided and time given for draining the saturated chalk. The small runs of water I saw in the French works in the lower chalk could not have existed without open fissures extending far enough into the saturated chalk to supply the water flowing from them, and as the pumping goes on underground closed fissures will open out and the discharge of water will increase as larger areas of saturated chalk are exposed on the sides of the fissures.

Whatever, therefore, may be urged as to the advantage of trying to follow these lowest beds of the chalk near the outcrop on the Shakespeare Cliff route in order to avoid water, which at the best is an uncertain advantage, would be more than outweighed by some very obvious and certain disadvantages and even positive dangers in attempting to do so.

The arguments set forth nearly fifty years ago for making the Channel tunnel in the beds of chalk hold good to-day. No new facts have come to light. Some shafts have been sunk at Dover through the chalk to the coal measures. In some of these much water was met with, in some little, which is what could have been foretold from our knowledge of the chalk.

From other causes the cost of making and of working the tunnel has been reduced. We have no longer to provide for the removal of the large quantity of carbonic-acid gas which locomotives produce. The traffic can be worked electrically. In addition to this, our knowledge of tunneling by machines through water-bearing strata has been increased by practice in the tunnels made under the Thames and for the tube railways.

Sir John Hawkshaw had satisfied himself as an engineer, not only that the tunnel could be constructed, but as to its utility as regards trade and commerce, and at the inquiry held by the Select Committee of the House of Lords and Commons in 1883 expressed himself to this effect. He was pressed very much by the committee to give an opinion as to whether the construction of the tunnel would be a source of danger to the nation, and while asserting that he had considered the tunnel from an engineering and commercial point of view, he maintained that the question of protection or defence was one for the consideration of the War Office, who had already, as I have stated, appointed some of its officers to visit Dover with the object of specially reporting upon the position of the entrance and the protection that might be considered necessary. Consequently, after the report of the Select Committee that it was not expedient that Parliamentary sanction should be given to a submarine communication between England and France, the whole question was naturally dropped.

All those who took part in the inquiry I have attempted to describe, and in the incidents it led to, excepting myself, have passed away. Lord Stalbridge, the chairman of the first company, who was the first boy I fagged for at Westminster School sixty-three years ago; the directors of the original Channel Tunnel Company, the engineers, my father, Brunlees, Low, and Ed. Easton, whose knowledge of working in water-charged chalk was so great; Day and Topley, the geologists, and my old friend, H. M. Brunel, who did such good work; Bellingham, our untiring secretary; Brunton, whose clever machine would have done the tunneling work; the contractors, Brassey and Wythes; and Crampton, who experimented for us on the transport of chalk in a fluid state. They are all gone; but if a Channel tunnel is ever made their work should not be forgotten, and especially my father's, on whose judgment they all relied.

* * * * * * * * *

Mr. J. Dobson, after reading the paper, said: "There are just a few remarks I should like to make in connection with the great scheme of the proposed Channel tunnel, which is again before the country. As a matter of fact, I was brought up (so far as the early part of my professional career was concerned) in the very atmosphere of the tunnel scheme, having had the privilege of serving the last year of my pupilage—1864 and 1865—with Sir John Hawkshaw, and for twenty-five years as one of his assistant engineers, until I became a partner of the firm. I well remember the preparation of the early sets of plans, each set consisting of five. Indeed, there were naturally so many sets to make that it seemed at the time as if we had to do with nothing but chalk. The cartoon was compiled from information supplied by Mr. Hartsinck Day to whom Mr. Hawkshaw refers, and is signed, as are the others, by John Hawkshaw, James Brunlees, and William Low. The plan in the Channel Tunnel Company's publication of December, 1912, is an exact reproduction of this plan, but without the signatures. Now I think we are much indebted to Mr. Hawkshaw for bringing before us the early history of the work done by Sir John with regard to this grand enterprise, which we all considered was, and would be, the great work of his life, for it is not well to forget those who have borne the burden and heat of the day, and especially the one who was the first practical pioneer of this scheme.

"There are just two points in the paper to which I should like generally to refer—construction and protection. First, as to the construction of the tunnel. Mr. Hawkshaw draws attention in his paper to Sir John's evidence before the Joint Committee of the Lords and Commons in 1883, when he stated that he had satisfied himself as an engineer that the tunnel could be constructed. But upon referring to the minutes of evidence, I also find this statement made by him—a statement which should go a long way to help the Channel tunnel forward to-day: 'If it is desirable that the tunnel should be made, I should be willing to risk my reputation if I am asked to undertake it and to make it.' Now it should be remembered that this statement was made in the early days, when tunneling was very different from what it is to-day, and before electrical working was proposed or thought of for the railways. Mr. Fell, the chairman of the House of Commons Committee, to whom we are much indebted for all he has done, and is doing, toward the furtherance of this great scheme, in his valuable paper recently read before this Society, drew attention to the fact that in nothing has greater progress been made in recent years than in tunnel building, and this is perfectly correct. Therefore, if Sir John Hawkshaw, in 1883, made the statement he did, how much more confident, owing to these great changes, would he have been to-day in his ability to carry it out. From the paper it is, I think, clearly shown that one of the great points in Sir John Hawkshaw's mind was that the tunnel, if built, should be in the lower or grey chalk. I am sorry to see that the proposed tunnels do not follow the line laid down by Sir John Hawkshaw, because his line (see map page 19) for the most part would pass through a thickness of some 480 feet of chalk practically in the center of the same, while that laid down by the present advising engineers does not appear to secure the advantage of so great a depth of grey chalk. Second, as to the protection of the tunnel. When the question of protecting the tunnel came up in 1883, we who were then Sir John's assistant engineers, used to discuss very freely how we would get rid of the invaders, and the general feeling was that the best way would be to flood them all out, and it is certainly interesting to find that Sir Francis Fox, in opening the discussion on Mr. Fell's paper, suggests the same method of protection. It is true that placing the entrance under cover of the guns, or flooding the tunnel, one would have considered more than sufficient, but this would have meant wholesale slaughter, such as would result from a submarine attack. Here again we have a proof that as time passes things change, for better or for worse—and certainly in this case for better—for to-day Mr. Fell has clearly shown us in his paper that there will be no need to protect the tunnel either by firing or flooding, because the French, to satisfy our military authorities, have agreed that the sole power-station shall be on the English side. Now, what does this mean? Simply that, by moving a lever the whole of the power for working the traffic can be stopped—temporarily or permanently. Much was said at the Joint Committee in 1883 as to the national element of danger which would arise if the tunnel was constructed; but surely if this is so, only additional precautions are necessary to counteract the danger, as is the case in every improvement where traffic is concerned. To illustrate my meaning, take the London traffic, and let us look back, say, fifteen years. We see the old four-wheel cabs, hansoms, and 'buses. Now, speaking for myself, I was thrown right out once from a hansom, and once the hansom was overturned with me inside. In both instances it was found the men were slightly the worse for drink, and it, unfortunately, was not a rare thing to find drivers in that condition. To-day we go by mechanically-propelled vehicles, which carry us at least twice as fast as the hansoms, and in the midst of traffic which is enormously increased. Can we expect to do so without incurring extra risk? Yes, we do. But why? Because, and no one can fail to observe it who will take the trouble to do so, the men who are driving the mechanically propelled vehicles are a totally different class—quick-sighted and energetic, and men who, as a whole, are first-class drivers, and I have never seen one the worse for drink; while the efficiency of the police in the control of the traffic—always good in the old days—is to-day, with this enormously increased traffic, perfectly wonderful. Therefore it is obvious that any supposed increased national danger, owing to the construction of the Channel tunnel, can be minimized by increased careful supervision, and if, as I have pointed out, by the movement of one handle the whole traffic could be stopped temporarily or permanently, and by the movement of another the tunnel could be entirely closed, should it be considered necessary, then is it to be supposed we cannot depend upon our men doing such a small thing as this? Surely even the thought is absurd, for if such were the case, I think we should deserve to be invaded, but may God grant this may never occur."

Radiation and the Atom

Sir J. J. Thomson's Investigation

Prof. Sir J. J. Thomson, O.M., president of the Physical Society, described, in the Cavendish Laboratory, to the members who visited Cambridge recently, the latest results of the experiments that he has been making on the production of very soft Röntgen radiation, with a view of investigating the properties of the atom.

The researches of Röntgen and his pupils, he said, had always connected-up light with electrical waves, or waves produced by what he might call mechanical means. There was a slight gap between the well-known red radiation and the shortest electrical wave that could be produced by the ordinary mechanical means. The study of Röntgen radiation had enabled them to prove the existence of two separate rings of electrons in the atom, one inside the other. The one was responsible for what was known as the K kind of radiation, and the other had the L characteristic. The L characteristic was so much softer than the K that if the rate of increase in softness was in anything like the same proportion, the radiation from a third ring would come well within that region of radiation which at present had not been studied. If they commanded a continuous series of radiations they would be able to see how many separate vibrating systems, how many rings of electrons, there were inside the other, and, more than that, they would be able, by the study of that radiation, to gage the number of electrons in each ring. This study, therefore, promised to give them the means of determining the distribution of electrons throughout the atom, and that was mainly the reason why they had lately in that laboratory been trying various methods of exciting that type of Röntgen radiation and studying its properties.

Two methods had been employed. The first was the production of Röntgen radiation by the impact of positively charged atoms. As far as he knew, no radiation due to the impact of positively charged atoms had yet been detected; but by taking advantage of the very remarkable sensitiveness of the Schuman photographic plate, they had been enabled to get unmistakable evidence that as the positive rays impinged against a surface they gave out a type of Röntgen radiation. A Crookes tube was employed for these experiments. His second method, he said, was on more orthodox lines, and in using it they had studied the effects of the impact of cathode rays, the speed of which was very much under their control. For these experiments an ordinary Röntgen-ray tube was employed. The photographic method, he continued, involved a considerable waste of time, and they had lately tried experimenting with a substitute for the photographic plate, and if they succeeded with those experiments they probably would be able to get on much more quickly. But even with the photographic plate they hoped to make a series of experiments which would enable them to find how many rings of electrons there were in an atom.—*The English Mechanic and World of Science.*

Graphite Against Furring

Various methods have been proposed and tried for preventing the furring of boilers. So far the best success has been obtained with those systems which prevent the deposition of scale right from its inception. In Germany many large firms of engineers, spinning mills, weaving mills, and other users of machinery, have found that graphite is a most efficient means to this end. In proportion to the size of the boiler, from 1 to 2 kilogrammes of graphite are placed in the boiler after it has been well cleaned. In addition to this 15 grammes of graphite must be added for every 100 liters of water evaporated. The process is as follows: The walls of the tubes, owing to the different temperatures prevailing in the boiler, expand in varying degrees and fissures or splits are thus continually produced in the furstone as it forms, and, into these, the graphite penetrates and finally covers the whole wall of the tube so that scale is no longer able to adhere thereto. Of course only highly pulverized, perfectly pure, graphite must be used.

California Led Last Year in Timber Sold from national forests, though Montana had the largest number of sale transactions.

Seventy-Six-Ton Steam Shovel*

Designed for Large Outputs and Heavy Material

We publish herewith illustrations of a 76-ton steam-shovel, of which several examples have recently been constructed by a Lincoln firm of manufacturers. The machine has been designed to deal with large outputs and heavy material, and marks the culmination of an experience extending over thirty-eight years. The same firm also build the crane-navvy, or full-circle type of machine, as well as the limited-swing steam-shovel of the class we now illustrate. Their range of crane-navvies covers weights from 18 to 60 tons, and their range of steam-shovels 20 to 76 tons. The machine we illustrate has been designed for large outputs and heavy material. It is normally fitted with a bucket of 3½ cubic yards capacity, but for exceptionally heavy or light material a smaller or larger bucket would be fitted. A machine of the type shown, excavating very heavy rock used in making Portland cement at a quarry in Aberthaw, and operating on a working face 30 feet deep, is loading 7-ton wagons in from one to one and a half minutes.

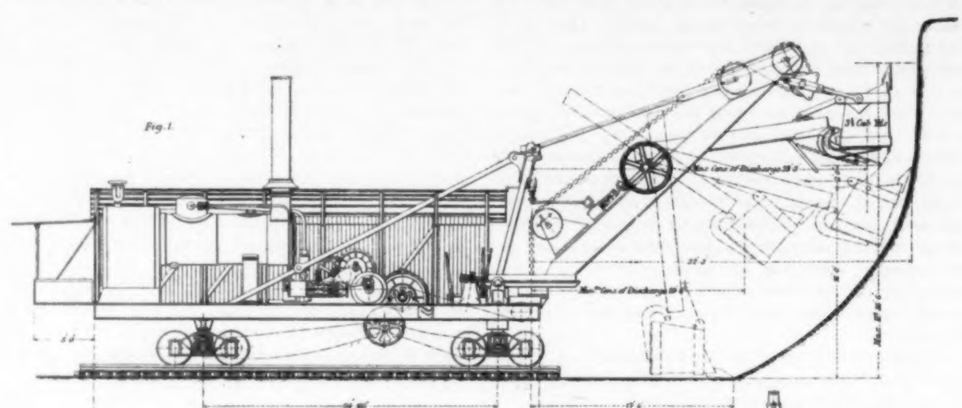
The general lines of the machine are well shown in Figs. 1 to 3. It consists of an under-frame built up of heavy joists, and plates are supported on two four-wheel bogies. The jib and its turntable are mounted at the front end of the under-frame and are balanced by the boiler carried at the other end. Separate hoisting and slewing-engines are mounted in the center. The traveling gear is operated from the hoisting-engine. The jib carries a bucket-arm, the movement of which is controlled by the main chain and by a racking-gear mounted on the jib. A rack formed on the under side of the bucket-arm gears with a pinion, which is gear-driven by a racking-engine mounted on the jib. This gear determines the projection of the bucket-arm from the jib, and hence the depth of the cut taken from the working face. The force required for cutting is supplied by the hoisting-engine, which hauls the bucket upward, the arm in the meantime swinging round the center of the rack spur-wheels. Various possible positions for the bucket and arm are indicated in Fig. 1, which gives the maximum digging height, range of discharge, etc. The bucket is discharged by releasing a door, which forms its bottom, this being controlled by a rope, which is operated by the man who looks after the racking-gear. Views of the machine in operation are given in Figs. 4, 5, and 6.

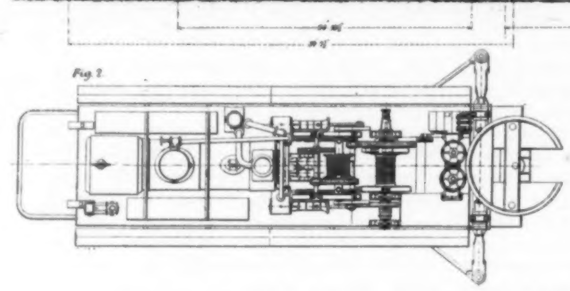
This brief description should be sufficient to make the working of the machine clear in a general way, and we may now proceed to deal with the details of its construction, leaving any further account of its method of operation to the end of the article. We may begin with the hoisting and slewing-engines, which are well shown in Fig. 7, and of which detail drawings are given in Figs. 8 to 10. These engines are to some extent built together, as will be clear from Figs. 8 and 9. The slewing-engine lies between the side-frames of the hoisting-engine, and the slewing-drum and second-motion shaft are mounted on these same side-frames, which carry the crank-shaft bearings of the hoisting-engine. The upper drum, i. e., the left-hand one of the two in Fig. 9, is the slewing-drum, the hoisting-drum being in front and at a lower level. The hoisting-engine has cylinders 10 inches in diameter by 14 inches stroke, and is fitted with link reversing gear. Crank disks are employed to add fly-wheel effect. The slewing-engine has cylinders 7 inches in diameter by 7 inches stroke. It is controlled and reversed by a single lever, which operates a change-over valve. The engine is practically a duplicate of the racking-engine which is illustrated in Figs. 11 and 12, and to which we refer below.

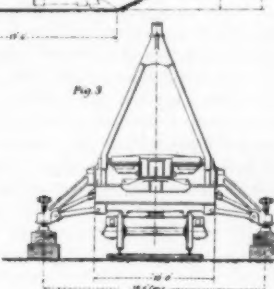
The operation of the hoisting and slewing-engines is under the control of the driver, who stands at the front of the machine close to the hand-levers, which can be seen in Figs. 1 and 2 and in Figs. 5 and 7. As is best shown in Fig. 7, there are three main levers: a smaller lever, a pedal, and a further lever on the far side of the platform, which can be seen to the left in Fig. 7. This far-side lever is for the purpose of operating the traveling-gear clutch. It is not used during the ordinary working of the machine, and is purposely placed well away from the control-levers proper. Of the three main levers, one operates the steam-clutch of the hoisting-drum, which we will describe in a moment, another controls the link motion of the hoisting-engine, and the third the change-over valve for the slewing-engine. The smaller lever, to the right in Fig. 7, is for the operation of the throttle-valve for the hoisting-engine, while the pedal is connected to the brake of the hoisting-drum. The driver has a further lever under his control, which is not shown in Fig. 7. It can, however, be seen in Fig. 5. It is pivoted above his head, and operates the throttle-valve of the racking-engine which is carried on the jib.

The steam-clutch is a very interesting feature of the hoisting-engine. It forms an easily operated and powerful gear by which the load can be taken up instantaneously upon the driver pulling over the hand-lever. It can be put into action with the engine running, and no special care is required, as the action is automatic as soon as the lever is pulled over. The clutch will transmit the full power of the engine and lift a test-load of 30 tons. It is well shown in Fig. 7, and is detailed in Figs. 13 to 15. As will be seen from Figs. 8 and 9, the crank-shaft of the engine drives the spur-wheel on the hoisting-drum shaft through a pinion. The spur-wheel is secured to the shaft, but the hoisting-drum is free from it. The hoisting-drum carries a clutch-drum at one end, which is embraced by a wood-lined clutch-strap. The strap is pivoted to the spur-wheel in such a way that when it is tightened on the clutch-drum the whole arrangement drives together. As will be clear from Figs. 13 and 14, the strap is tightened or released by the movement of a toggle which is operated by a bell-crank lever lying at right angles to it. The bell-crank lever is pivoted to the spur-wheel, and the end of its inner arm lies in a slot in the shaft. A steam cylinder is mounted at the end of the shaft and axial with it, as is well shown in Figs. 7 and 14. The cylinder piston-rod is directly connected to a plunger working in a hole in the end of the shaft, the plunger having a slot at its inner end, in which the end of the bell-crank lever-arm fits. It will be clear that the movement in or out of the cylinder piston will move the bell-crank lever, and so expand or release the clutch-strap. The piston is controlled by a simple slide-valve, which admits steam to either end of it, the valve being operated by the hand-lever already mentioned.

At the other end of the hoisting-drum from the steam-clutch there is a brake-drum with a wood-lined band-brake, operated by the foot-pedal already mentioned. This is used to sustain the load while the jib is slewing preparatory to the discharging of the bucket. The main drum itself is grooved for the hoisting-chain, as shown in Fig. 1. It works on long brass bushes. The traveling-gear for the machine is worked through a spur-gearing, situated at the brake end of the hoisting-drum, and shown in Figs. 8 and 9. This gear is thrown into connection with the main shaft by a dog-clutch when it is desired to bring it into use. From the







* Reproduced from *Engineering*.

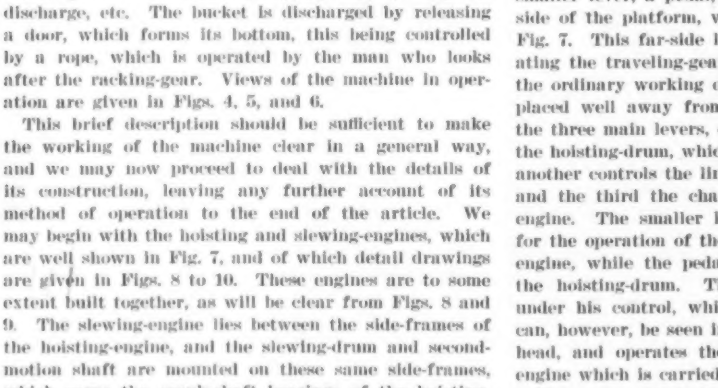


Fig. 4.—Shovel working in a chalk quarry at Hessle.

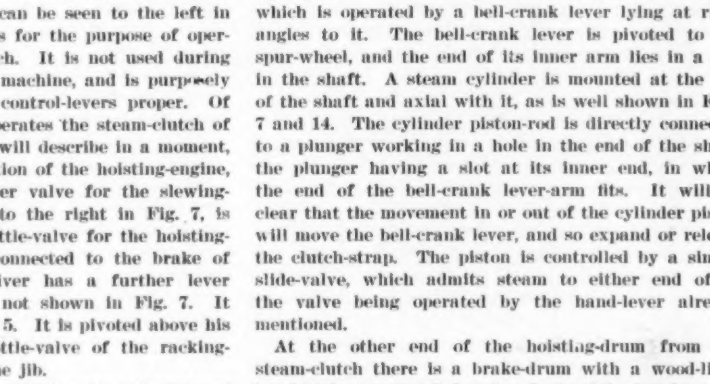


Fig. 5.—Shovel working in a chalk quarry at Hessle.

Fig. 6.—Shovel excavating at Hull joint dock.

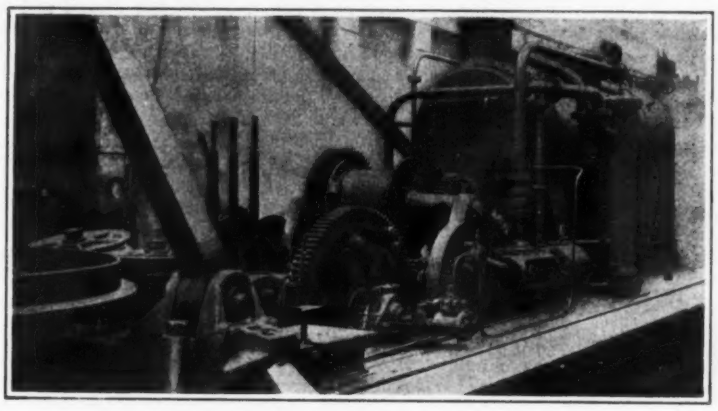
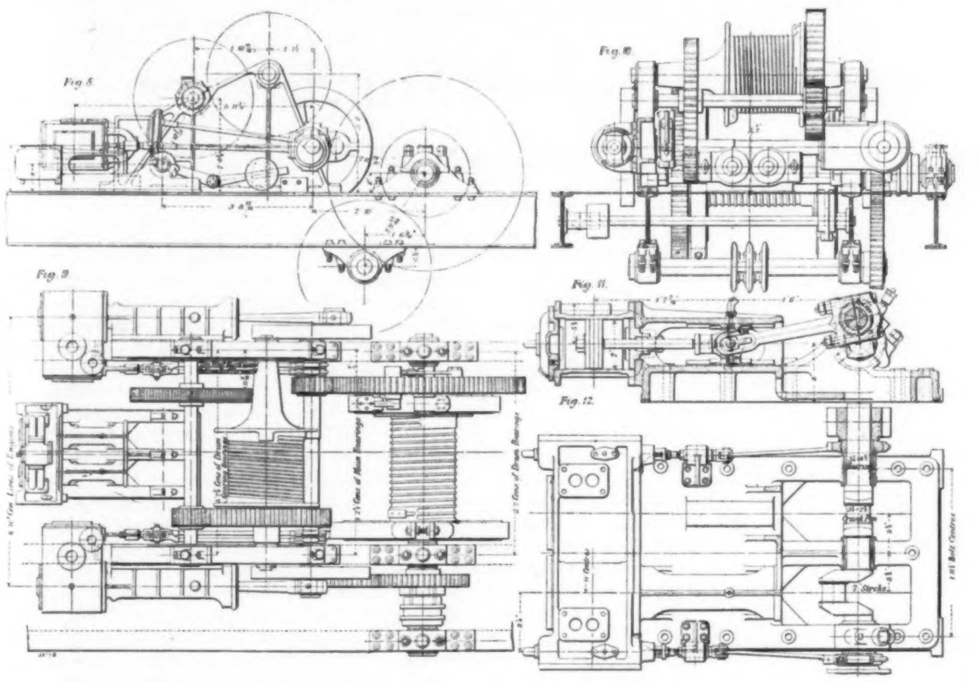


Fig. 7.—View showing hoisting-engine and boiler.

second-motion shaft of this gear, power is transmitted to the traveling-wheels by flat-link chains, as shown in Fig. 1. The chains allow for a certain amount of side swing of the machine. The bogies are of the diamond-frame type, of specially heavy construction, and heavy compression springs are fitted to absorb the stresses due to digging and traveling. The bogies are best shown in Fig. 4.

The boiler is of the locomotive type, which possesses many advantages for this class of work, of which not the least important is that steam can be raised quickly. The design allows ample facilities for cleaning, which is an important point, as frequently bad water may have to be used. The working pressure is 125 pounds per square inch, and the hydraulic test pressure 220 pounds per square inch. A special feature in the construction of the boiler is the method adopted for staying the fire-box and the foundation-ring. The pressure on, and weight of, the fire-box are taken from the foundation-ring by a system of radial staying of the boiler-shell. The foundation-ring is made from Z-bar. There is only one weld in the frame, and the corners are bent in an hydraulic press. The combination of the radial roof-stay and the Z foundation-ring gives an elasticity which amply provides for the breathing of the fire-box. Steam for each of the three engines is taken from the steam-dome by a separate pipe. Each pipe is fitted with a stop-valve and regulating valve.

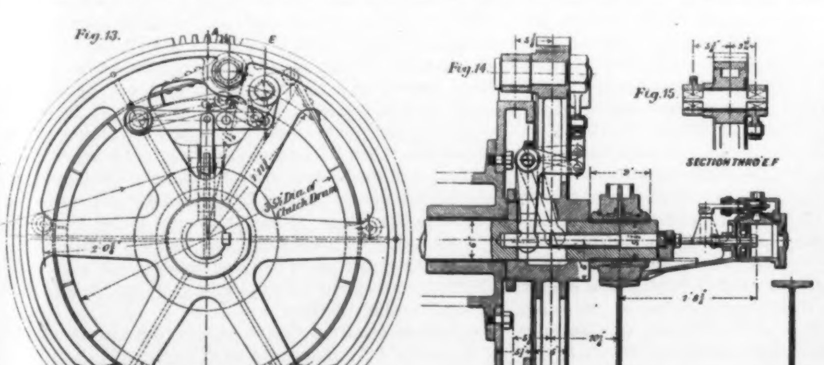


To reduce fuel consumption and avoid strains caused by cold-feed, an exhaust-steam feed-water heater is used, the temperature of the feed being raised to nearly 212 deg. Fahr. Two water-tanks are fitted, one on each side of the boiler, which is mounted centrally on the framing, as shown in Fig. 2. This figure shows the tanks, each of which holds 750 gallons. Feed is by a duplex pump. The boiler is fired from a platform at the rear end, as shown in Fig. 4, and the whole of the body of the machine is covered by a housing as a protection against bad weather and flying material when blasting is being carried on.

The jib, as will be seen from several of the illustrations, is mounted on a turntable carried at the front of the machine. The turntable is built up of mild-steel plates and angles, into which a heavy steel casting is fitted to receive the foot of the jib. The whole rotates on a steel pivot casting bolted to the main framework. Slewing is performed by two wire ropes which pass round the rim of the turntable, and are carried through guide-pulleys to the spirally grooved slewing-drum. The guide-pulleys can be seen in Figs. 1, 2, and 7. In addition to the heavy steel casting forming the pivot there is a further casting built into the front part of the machine. This forms the base of the A frame, which carries the tie members supporting the outer end of the jib. The A-frame legs are solid mild-steel forgings of square section. At the bottom they are pivoted to the casting mentioned above, as is clearly shown in Fig. 7. At the top the A-frame members are bolted to another steel casting, on which the tension-stay crosshead rotates. A back stay connects the top of the A frame to the under-frame below the boiler. The casting to which the bottoms of the A-frame members are pivoted carries two brackets bolted to its ends, one of which is well shown in Fig. 7. To these brackets are pivoted the main members of the jack-arms which serve to take the overturning forces which come into play when the jib is swung to one side or the other. The legs are shown in Figs. 2 and 3, and in all the views of the machine in operation. As will be seen, they carry adjusting-screws at their outer ends, which rest upon wood blocks or packings. When the machine has to be moved the screws are slacked back, so that the packings can be removed and reset in the new position. When traveling, the jack-arms are folded against the sides of the under-frame by removing two pins and releasing the connecting-links.

The jib is illustrated in Fig. 16. It consists of two oak members reinforced on all sides with mild-steel plates. This form of construction has been found to be the best to withstand the severe stresses set up by slewing at a high speed with a loaded bucket. Heavy mild-steel plates and angles are riveted to the top of the jib to carry the chain-pulleys and take the ends of the tension-stays. A mild-steel plate is fitted to form a seat for the racking-engine, while a similar plate carries the bracket for the lower chain-pulley. The bolts for the racking-bearings and the engine are taken right through the jib. The racking-gear is operated by a double set of spur-gears driven direct from the engine crank-shaft. The racking-shaft, which carries the spur-wheels and racking-pinion, is of large diameter and is formed with squares for the wheel-seats instead of keys being used. This eliminates possible trouble through loose keys. The racking-engine is illustrated in Figs. 11 and 12. It has cylinders 7 inches in diameter by 7-inch stroke, and, as before stated, is controlled and reversed by a single valve. Reverse is obtained by altering the direction of flow of steam to the engine, the steam-pipe being made the exhaust-pipe, and *vice versa*.

The bucket-arm is shown in Fig. 17. Like the



Figs. 13 to 15.—Steam clutch.

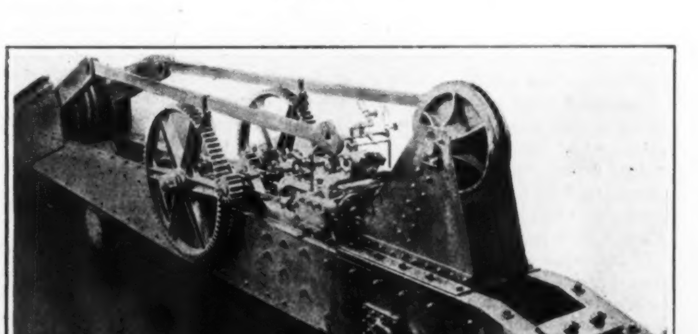


Fig. 16.—Jib looking from bottom end.

jib, it is built up of oak reinforced with steel plates, the rack being a steel casting. The arm is kept in position on the jib by a guide-plate, which is pivoted about the same center as the racking-pinion. The plate can be seen in Fig. 5. The arrangement allows the arm to swing to any necessary angle, as shown in Fig. 1, and allows it to travel endwise, while at the same time keeping the rack always in gear with the pinion. The bucket is built up of mild-steel plates, while the arm, braces, etc., are steel castings. The teeth, which are carried almost the full depth of the bucket, consist of mild-steel shanks with renewable manganese-steel points, and are fitted on a renewable lip-plate. The teeth can be renewed in half an hour. The hinged bottom door of the bucket is provided with a catch, which can be released for discharging by the jib-man pulling a rope, as shown in Fig. 6. The door automatically closes and locks itself after discharging, as the bucket-arm is lowered into a vertical position for the beginning of a new stroke. The jib-man stands on the small platform carried by the jib, and also controls the racking-engine by operating the change-over valve. Steam is conveyed to this engine by an overhead pipe, which has a gland joint to allow for the slewing of the jib. The pipe and joint can be seen in Fig. 1.

Of our three illustrations of the steam-shovel in operation, Figs. 4 and 5 show a machine in a chalk quarry at Hessle, near Hull, and Fig. 6 shows a machine working on the new Hull Joint Dock. We have had an opportunity of inspecting the Hessle installation. The chalk was of quite a hard nature, but the

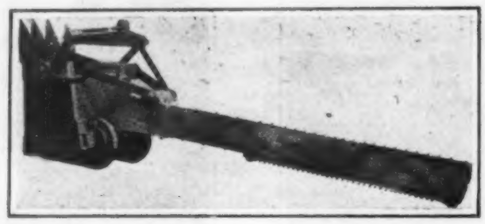


Fig. 17.—Bucket arm and bucket.

shovel dealt with it easily. The material was dumped by the bucket into trucks brought up alongside, much as in Fig. 6. The arrangement by which the three main motions are dealt with by separate engines appeared to be excellent, and the control of the racking and hoisting worked admirably. The situation of the man on the jib-platform enabled him to see exactly what the bucket was doing, so that he could regulate the extension of the bucket-arm to a nicety, while the easy control of the steam-regulator enabled the driver to humor the bucket up the working face if it came in contact with large bolders or other obstacles. There appeared to be not the slightest difficulty in so controlling the machine that a full bucket was obtained at every stroke. The slewing was very rapid, and the shovel could have delivered material considerably more rapidly than the wagon service could carry it away.

Phosphorescent Calcites

M. Lacroix, professor at the Paris Natural History Museum, has presented before the Academy a very interesting paper by M. Pisani, who has observed that certain crystals of carbonate of lime can, under the influence of heat, acquire a remarkable phosphorescence which renders them luminous in the dark. Up till now it was thought that this phosphorescence was due to the presence of small traces of rare bodies. In the majority of calcites there are to be found, indeed, extremely small quantities of yttria; but M. Pisani has operated on absolutely pure crystals of carbonate of lime, and he has observed that when they are heated they become phosphorescent. This curious property of the crystals of carbonate of lime is, then, not due to the presence of rare elements.—*Chemical News.*

The Patent Expert and the Chemical Manufacturer*

The Expert's Function in Preventing and in Conducting Patent Litigation

By Bernhard C. Hesse

By "patent expert" I do not mean that professional man who is called in by a litigant only at the critical or crucial stage of a controversy long under way and most usually then in its final stages. On the contrary, I mean that professional man who is not only a highly trained and well equipped chemist, but who has also a natural or an acquired bent of mind which enables him to collect and assimilate the chemical and other facts relevant to the matter in issue, arrange them in logical order, survey them critically, and prepare them for submission to legal analysis and examination by patent counsel, and who makes that particular occupation his vocation in life.

There can be no question that actual patent litigation between inventors or their principals is an unhealthy and an abnormal condition and is thoroughly disadvantageous to the commercial and industrial development of chemical inventions. The energy and ingenuity consumed in litigation could be applied much more profitably to the development of the enterprise itself than to a controversy over where the rights of the one begin and the rights of the other end.

It is perfectly safe to say that the great majority of inventors and their principals are anxious and willing to respect the rights of competitors. They are, however, hampered in the practical expression of this willingness by ambiguity, uncertainty, and error in the statement of supposed rights as expressed in patents. It is to the elimination of these defects that the patent expert must chiefly address his efforts and thus ultimately justify and vindicate his activities and his position in this work-a-day world.

There can be no question that the most economical and profitable employment of a patent expert is at and during the development of a chemical invention itself—not after the patent has been issued and is in litigation. His chief and most useful function is to reduce the likelihood and cost of litigation to a minimum; this he can do only by a careful and patient examination and arrangement of all the relevant facts and by their proper submission to lawyers for final judgment, *before* the application is filed in the Patent Office.

This is merely a specific application of the old adage: "An ounce of prevention is worth a pound of cure." While this may seem to many an obvious and self-evident course of procedure, yet only a minority of the chemical manufacturers and inventors in this country have regarded that course as being, in the long run, the advantageous and proper one for them to follow.

Anyone who has actively participated in strenuous patent litigation and has had his share of responsibilities to sustain, knows the feverish excitement and dissatisfying conditions due to hard work under extreme pressure, the extraordinary and unusual demands made upon the working staff of the litigants from the highest official down, and the consequent unavoidable interference with the regular occupation and operation of staff and works in many, if not all, their divisions. He is also thoroughly convinced that any and all steps taken at the inception of the cause of trouble, namely, the invention and the patents based thereon to prevent such congestion and such high-pressure work must be, in the great majority of cases, far more profitable, economical, and efficient than any attempt at correction or avoidance after the trouble has begun.

It would be idle to expect or to hope that all controversies as to the beginning and ending of rights could be eliminated by such careful preparation of a patent. That the points in issue would be reduced to a minimum is certain, and it is equally certain that the meritorious issues in a case would not be smothered in a mass of minor, technical, or irrelevant disputes, all, or at least the great majority of them, avoidable by care, caution, and patience in the draft of the specification. The smaller the number of such minor points in a patent suit, the shorter the suit and the less expensive to the litigants, while the court and all others concerned are given an opportunity to concentrate attention and effort upon the points that really are meritorious and which alone should count.

It is safe to say that in the average chemical patent suit anywhere from 25 per cent to 50 per cent of the total litigation cost could have been avoided at the outset by proper, complete, and non-ambiguous drafting of the specification and its claims and careful scrutiny thereof after allowance and prior to issue of the patent. A patent conservatively drawn, complete, clear, and full in its disclosure as well as clear in its claims, is far more efficient as a protector against infringement of the invention involved and more certain of favorable adjudication than one not so drawn.

Chemical cases have been litigated in which a misplaced decimal point caused 8 per cent, a superfluous adverb consumed 12 per cent, an incorrect and superfluous theory used up 20 per cent of the total cost of litigation, and finally, had a certain disclosure been just a little bit more explicit, the litigation would not have been started at all. In still other cases, had the relevant art been searched with an open and critical mind prior to patenting, there never would have been any litigation. Further, the number of patents that have been rendered ineffective because of improper statement of invention, insufficient or incomplete disclosure is very great, and most of these defects would, or should, have been avoided in the issued patent had there been suitable technical supervision and criticism such as by a patent expert. It is certainly easier and far less harrowing to examine and criticise an application, even if it be your own work, than to have to sustain an issued and faultily drawn patent when you have no chance for correction or alteration, but must stand or fall by the document, "as is."

The pitfalls are many; there is no really dependable chart; each case must be treated on its own footing. The more careful the search, the more cautious the judgment, and the fuller the knowledge of the relevant facts, the greater the fullness of disclosure, the more circumspect the phraseology and the greater the clarity of expression, the greater are the chances of success in avoiding useless points of attack, in minimizing effort and expense during litigation, and the greater the protective value of the so-resulting patent to a meritorious invention. No amount of bolstering or shoring up *will* or *should* permanently help a non-meritorious invention.

THE PATENT CHEMIST—WHAT HE IS.

The man whose business it is to attend to these matters I have referred to as a patent expert. This appellation is in itself something of a handicap to him in his work. There is no real reason why his expertness, real or assumed, in his special field should ordinarily be emphasized any more than in every-day life a skilled chemist is burdened with the designation of "expert." This man's real business is to be part and parcel of creative organization and machinery, not a man apart. Why not call him "patent chemist"? We have leather chemists, paper chemists, sugar chemists, and the like. The patent chemist is one who specializes in the chemistry of patents, and patents in chemistry. Calling him "patent chemist" makes him on the surface at least more nearly part and parcel of the working staff than does the designation "patent expert"; the former name invites familiarity and co-operation, and suggests utility and work, all of which is only helpful to those concerned, whereas there is a certain amount of aloofness or apartness, a suggestion of extraordinary and formal occasion and surroundings, of so-called "ornament," unconsciously, but none the less surely, associated with any term involving the word "expert."

THE PATENT CHEMIST—WHAT HE DOES.

Now, this patent chemist, as I have called him, what does he do, and how does he set about to accomplish it?

His usefulness begins with the inception of an invention and continues until the last bit of litigation is put out of the way, successfully or otherwise. He begins by getting a close understanding of the invention by careful, exhaustive, and analytical study of the relevant prior art; he determines in his own way the presence of invention, defines the scope and nature of the invention, directs or requires confirmatory or exploratory work in determining and settling its scope; in other words, he formulates and "proves up" the statement of invention. Then he must see to a full and complete disclosure, and finally a proper wording, classification, and subdivision of the claims. His next move is to take his tentative handiwork to patent counsel to see how well or how poorly he has constructed his work; often working together they ascertain and locate weak spots and determine what shall be done to clear up, define, and crystallize the situation.

THE PATENT CHEMIST, THE MANUFACTURER, THE INVENTOR, AND THE LAWYER.

So far, the patent chemist has acted largely as an avenue of communication between the inventor on the one hand who is a chemist, and patent counsel on the other hand, who is generally not a chemist. He must, in many cases, exercise great patience and ingenuity in

* Paper presented at the forty-eighth annual meeting of the American Chemical Society, Rochester, and reprinted from the *Journal of Industrial and Engineering Chemistry*, Vol. 5, No. 10.

getting the chemists's story into shape to lend itself to legal treatment, and on the other hand, he must reduce the lawyer's story to such terms that the chemist understands and appreciates the situation.

More frequently than not, the patent chemist must hold the balance true as against the "fond-parent" enthusiasm, the disdain for real proofs and the airy, generalizing tendencies of the inventor, the aggressiveness of the principal and the pessimism or cynicism of patent counsel, keeping his judgment calm, his reasoning sound, and his facts straight throughout all this, not infrequently, very turbulent and trying period of patent-development.

The amount of labor, effort, study, investigation, re-examination, collecting of new facts or proofs, and re-stating of positions and viewpoints required of a patent chemist to reconcile these three elements is at times very great and difficult, and always very trying, absorbing, and engrossing.

To establish the usage of a certain expression in chemical publications may seem, at first blush, to be a perfectly simple matter, but before the attempt to establish any particular usage has gone very far one is overwhelmed, more often than not, with a multiplicity of usages, and choice becomes difficult, if not impossible.

The question of what a publication, say forty years old, meant in whole or in part to the man then writing it, to a man reading it thirty or forty years later with all the intervening information at his disposal, is one whose correct answering may or may not interest the inventor or the principal, but patent counsel may know, and the patent chemist must not only get that answer, but he must prove every part of it. The questions of analogy, homology and the like, and their influence upon predication of invention are questions not always easy to answer, but whose answers patent counsel needs, and the patent chemist again must supply and prove in their every part.

Practically all of the great variety of puzzling technical questions which conscientious, capable, and competent patent counsel will propound in the course of developing a patent for a chemical invention resolve themselves into one or the other of the three above given, and the road to the answer is not unusually long, hard, or rocky.

Then comes the question of the accuracy, completeness, fullness, conciseness, and clearness of one's own disclosure, the arguing back and forth over this expression and that expression, this or that sequence of ideas and so forth; the same is repeated on the statement of invention, and finally all has to be gone over again when the claims are taken up.

This kind of work is no occupation for a real chemist, nor an inventor, nor a principal. It is too slow for any of them; their entire mental attitude and habit of thought would have to be changed from an enthusiastic, creative state of mind, based upon an abundance of relevant information to a cold, impersonal, analytic mental attitude based upon information, much of it not wholly relevant, but seemingly wholly foreign to the case; from the joyous and impetuous contemplation of his own creation the inventor would have to drop down to a very chill criticism of his own work as if it were the work of a total stranger. That is for most inventors a practical impossibility, and it is only natural that it should be so.

Essentially the inventor in the usual course of his occupation must take chances—otherwise he would not be an inventor; essentially, those concerned in securing by patent what the inventor has achieved must take no chances, and must be sure that no chances are being taken. It would certainly be extraordinary if both functions were to be successfully united in one and the same individual. Moreover, it as frequently as not takes more labor and effort to get a satisfactory specification together, than it takes to make and operate the invention. Very often, things on paper look very different from the real thing, but it is a most difficult operation to reduce those differences to comprehensible and concise written language. The average inventor would rather follow up the practical realization of his invention, or start something new and fresh, than be obliged to go over and over the same old trail straining his eyes for something he does not care to see, and which does not hold nor grip his interest. His particular work is done; it is up to others now—the patent chemist and the patent counsel. They must dig into the relevant chemistry and the relevant court decisions, write and rewrite the specification and claims until, in their judgment, all foreseeable contingencies are provided for and taken care of.

THE PATENT CHEMIST AND THE PATENT OFFICE.

At last, the specification is filed in the Patent Office, and quite as often as not, the first Office Letter will show the patent chemist that he did not provide for all chemical contingencies, and the patent lawyer that he did not provide for all law contingencies, or in the event they have done both, they evidently did not succeed in saying so in a manner that *could not* and *would not* be misunderstood. Then the work must be taken up anew; explanatory letters must be written, and many times these are not sufficient, and personal interviews are needed to uncover the cause of the misunderstanding. Here again the patent chemist must take up the technical side of the case, which, as a rule, he can present conclusively far better than the patent lawyer, just as the patent lawyer is far better able to present conclusively the law involved. Experience has shown, however, that law points are then far less frequently involved than are technical points.

If now the application, when in allowable shape, is put into interference, it is the patent chemist who must in the last resort decide if the proposed interfering claims are, or are not, such that his case can properly make; then the details of the interference proceeding and the preparation of the technical testimony, direct and cross, for and against, should all pass the patent chemist's scrutiny. The quality and amount of work required of the patent chemist in such proceedings is dependent almost wholly upon the caution and alertness exercised by him during the development of the application.

When finally a patent is issued and negotiations for acquisition of rights thereunder are taken up, it is the patent chemist who must expound the technical aspects of the subject to counsel for the other side, and must again and again defend his work.

THE PATENT CHEMIST AND LITIGATION.

However, the work of the patent chemist so far is a mere prologue to his work when a suit for infringement of patent is under way. Here is where he at times becomes actually the right hand of counsel, and the real test of his ability and preparedness takes place. He must sift and test the evidence of infringement, he must scrutinize and forecast all possible and probable positions of his opponents, must have the entire mass of facts and data at his tongue's end; in fact, he must be a walking and living dictionary, guide book, and cyclopædia not only through the particular art and case involved, but into the most refined and subtle distinctions in any and every branch of chemistry which even remotely touches the subject involved. The patent chemist frequently has the fate of the entire case intrusted to his keeping, and his success depends not only upon how carefully he has prepared his case, but also upon the celerity with which he can produce his proofs and his alertness in anticipating or forecasting the moves, near or remote, of his opponents, and preparing for them betimes. He must be able to do his work quickly and surely not only in the quiet of his laboratory or study, but more often under the strain of proceedings actually in progress and in the presence of his opponents. Not a single phase of the entire case must escape his attention and scrutiny. In one litigated case there was a total of 408 different chemical statements, for each of which all the relevant facts had to be collated from the literature and the relevant testimony on both sides tabulated for use as the case progressed; the subject matter was relatively simple. What would have happed had that subject matter been really complex is wholly a matter of conjecture and fearful to contemplate. Other and more complicated cases have entailed quite as much, if not more, diffuse and widespread labor. Certainly no inventor wants to be pestered with such, for him, dry-as-dust details.

If the crucial test of a patent be its ability to withstand onslaught in the courts, then the crucial test of the utility of a patent chemist is his ability to handle the vast amount of chemical facts involved with alertness, celerity, and accuracy on such occasions. This will be rendered more certain and of a higher degree of efficiency, the greater the familiarity of the patent chemist with the subject-matter, and generally this familiarity is the greater, the longer the patent chemist has been associated with the subject. The same is true of the patent lawyer. Upon this assumption it further follows that the only wise policy is to commit the drafting of the specification and its prosecution in the Patent Office from the very start to that patent chemist and to that patent lawyer to whom the defense of the patent in the courts is to be finally intrusted. Let these men select the ground on which a dispute, if any, is to be conducted while they have an opportunity of so doing; let them shape the course and form of the document over which a struggle is expected, and the results will be far more satisfactory than if those who are finally called upon to defend have no choice in the matter, but must take things as they find them.

It is true that only a very small fraction of the issued patents is ever brought to the supreme test, and it would be a very wasteful policy indeed to expend upon patents of obviously little intrinsic value the same amount of labor that would be called for by a very valuable patent or set of patents. But as to a patent or patents of value there can be no two opinions as to the best general course to pursue; let those who must ultimately do the defending select their own ground while they may.

Many suits for infringement of patents are started or are proposed to be started, many more than actually find their way to trial in the courts in the preparation of the technical matter (both offensive and defensive); in such cases the patent chemist must clear up and maintain clarity in the technical questions involved, because in such informal proceedings success demands completeness, celerity, and alertness to almost the same extent and degree that the more formal court proceedings do. Many a contemplated litigation has not been started because of precisely such proper preparation of material prior to and during negotiations looking to amicable adjustment.

It is not only natural, but inevitable, that the state of known facts changes and shifts and becomes fuller as the inventive idea and the patent pass through the different stages just outlined, and therefore judgment and opinion must frequently be tested and re-examined; these constitute the real cause for keeping the patent chemist in very close touch, in the majority of cases, with the growth and development of the invention, as well as with the business and all other similar conditions surrounding it.

THE PATENT CHEMIST AND THE CHEMICAL FACTORY.

From this sketchy outline of the patent chemist, his field, his mode of operation, and his relation to the manufacturer, it is no doubt clear that he is a man who must look at his chemistry not only with the eye and the mind of a chemist and of a manufacturer, but through the spectacles of a lawyer as well; he must look at patent law with the eyes of a chemist and the mental attitude of a lawyer and translate the law into chemical terms; he must know how to get convincing and correct answers to questions of great variety and scope, many of them seemingly trivial and simple, but at times of the utmost importance. He is neither a producer nor a creator of things; he is perhaps nothing more, in the final analysis, than a catalyst—a catalyst enabling two or more different agencies to operate in harmony and in complete understanding with each other and thus to increase the speed with which the object aimed at is achieved, and with generally beneficial effect upon the quality of the final product. He may also be regarded as a foster-parent to the children of the brains of others, and his function is to aid in their protection while in development. He is perhaps nothing more than an additional insurance against error in making plans for the future, and his value grows with the value of those plans. He is not a lawyer, nor is he a real chemist, but he must be primarily and fundamentally a chemist with a chemist's instinct and a chemist's sympathies; he must have a working knowledge and an appreciation of all business conditions likely to influence the course of development of any and all of the inventions with which he is brought into contact; he is a mixture of chemist, manufacturer and lawyer, and he must have an instinct and judgment for determining the correct time for, and the proper men to whom special questions must be submitted for final treatment. He must be especially alive to his own limitations and to those of others; he must not be unalterably wedded to his own opinions; he must be able, on occasion, to obliterate his own personality and to pocket his pride.

Now this brings me to the question of the status of the patent chemist in the organization or staff of a chemical factory. There can be no question that he must be in the confidence of the concern much more than the routine or works chemist; he must have greater freedom of action, greater radius of activity, and his information should be first-hand wherever and whenever possible. He can make himself useful not only as above outlined, but also by keeping systematic track of what competitors at home and abroad are doing as foreshadowed in the technical press, patent applications and issued patents in all countries, thus anticipating attempts to blanket or forestall his friends, but he may very often also be able to call to the attention of his principals new fields of endeavor and ways and means of entering them, which but for his watchfulness might escape notice. He should be made use of at every new manufacturing or operating step of his principals.

As to his position, should he be definitely inside or outside the organization, the answer is that it depends upon circumstances. In Europe, in some of the chemical branches, the patent chemists are fixed members of the organization, while in others the patent chemists are in business on their own account. Just how each or any organization shall handle that question involves the same questions as does the acquiring of any other commodity or service—by exclusive contract, by provisional contract, or in the open market. Each concern must choose and decide for itself.

To put it in a very few words, the chief function of the patent chemist is to apply Davy Crockett's rule, "Be sure you are right, then go ahead," to chemical inventions—a task not always interesting nor pleasant, but always useful and bubbling over with worth-while work.

The Manufacture of Crucible Steel*

Its History and Technology

By George H. Neilson

The pioneers in crucible melting are said to have been the Chinese, who used the process many centuries ago. But the art in China never progressed beyond the initial stage. The real father of the crucible steel industry was Daniel Huntsman, of Sheffield, Eng., a clock-maker, who found it impossible to get uniform steel from which to make his springs, and he hit on the idea of fusing blister steel in a crucible. This was in the latter part of the eighteenth century, and the melting of crucible steel has changed but little since that time. The details have changed somewhat but the actual process is much the same.

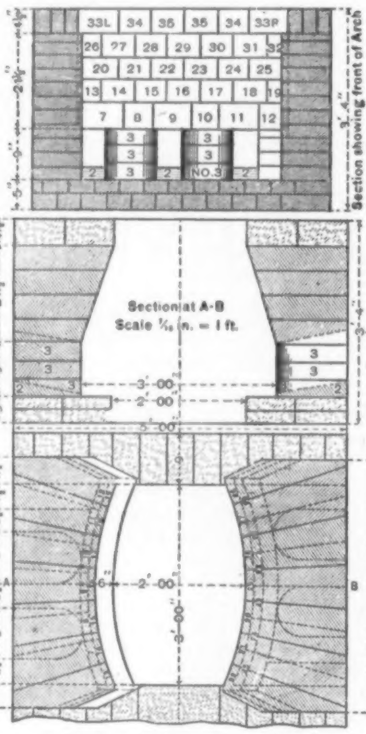


Fig. 1.—Detail of hole in a crucible furnace.

The material to be melted is loaded in a crucible, covered with a cap to keep out the gases, and placed in a hot hole and left there until melted. The crucibles have changed, the holes also have been changed in shape and size, and the method of heating is not the same, but the process is practically unchanged. Clay crucibles were the first of which we have any definite knowledge. They held about 50 or 75 pounds and lasted but one heat, and very often cracked and went to pieces before the steel was melted. Clay crucibles of the present day are much more durable and are extensively used in Europe, but little in this country. They have one decided advantage over the plumbago, or graphite, crucibles inasmuch as they do not throw off any carbon during the melting process. The plumbago crucible, which is the most generally used in this country, consists of about equal parts of plumbago and clay. The greater part of the plumbago is imported from Ceylon. These crucibles are capable of withstanding a very severe heat, and can be used a number of times, depending greatly on the nature of the mix and also whether the crucibles are replaced in the furnace before they get cold. The usual practice is to get as many heats as possible from the crucible without letting it cool. As soon as the melted steel is poured out, it is re-charged by hand, or by means of a mechanical shaker, and the crucible returned to the melting hole.

Furnace. The modern crucible furnace is of the regenerative type and is heated by gas, generally producer gas, although where natural gas can be obtained it is often used. Natural gas is probably a more costly way to run a furnace, but it has many advantages over producer gas. It is easier to regulate, as the flow is constant, which is not the case with producer gas unless a large holder is used. It is free from the poisonous fumes of the producer gas and is much cleaner. I am not in a position to say whether or not it is harder on the crucibles and furnace than producer gas. The capacity of a furnace is spoken of in pots. That is a 24 pot, 36 pot or 60 pot furnace. That is the number of crucibles the furnace will accommodate at one time. The furnace holes, in which the crucibles are placed, hold 6 crucibles, so a 36-pot furnace is one of 6 holes. The gas enters the holes at the bottom on one side, mixing with the air immediately before entering the melting hole, and passes out at the opposite side and then through checker work to the stack. When the valve is reversed, the direction of the flow of the gas is reversed. This is done every 15 or 20 minutes, and, in this way, the checker work on both sides is kept hot. The gas should not be pulled through the melting hole too rapidly. If it is, it will cut the port holes and also cut the crucibles. The gas should fill the melting hole and show a small flame around the covers. This is a sure indication that the gas is getting around the crucible and not pulling across the bottom. The detail of the hole is shown in Fig. 1, and a modern 36-pot furnace in Fig. 2.

* Address of the Retiring Chairman of the Mechanical Section of the Engineers' Society of Western Pennsylvania, published in the February, 1914, Proceedings, page. 90.

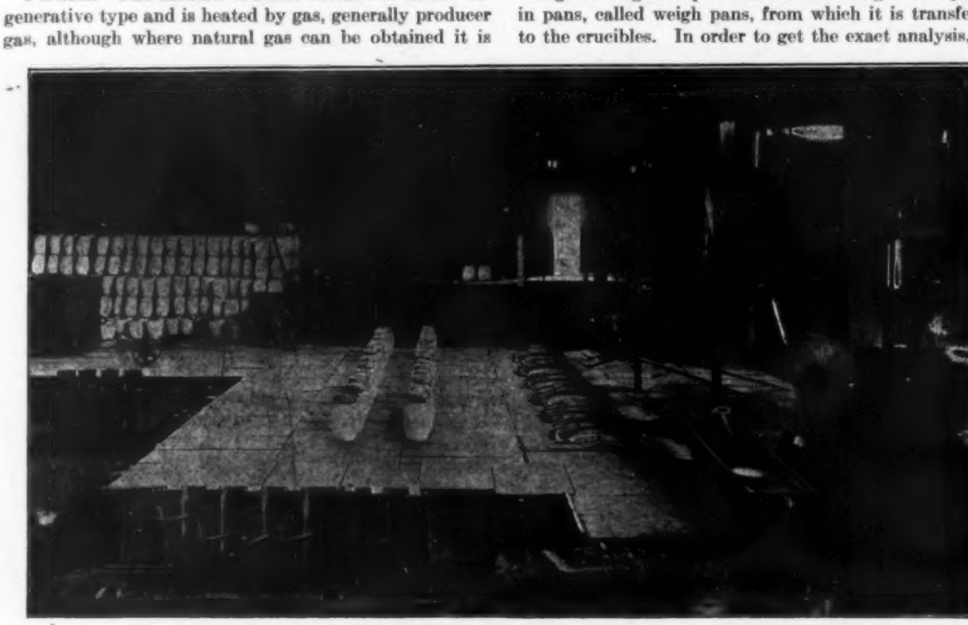


Fig. 2.—A 36-pot furnace.

Charge or Mix. The basis of good crucible steel is iron, and, consequently, the better the iron the better will be the steel. Therefore, it is vitally necessary that iron low in phosphorus and sulphur be used. As the crucibles generally in use hold from 100 to 125 pounds, the mix or charge is weighed up in lots of that weight and placed in pans, called weigh pans, from which it is transferred to the crucibles. In order to get the exact analysis, the weighing must be carefully done, in many cases to the exact ounce. When the crucible is filled it is covered with a cap. This is done to exclude deleterious gases which otherwise would impregnate the steel. When the material to be melted is weighed up, the amount of carbon given off by the crucible must be taken into consideration. If this is not done, the carbon content of the ingots will run higher than expected. The new pots, as a rule, do not throw off as much carbon as they will the second time used, and after the third heat the amount thrown out will be immaterial.

Melting. The length of time necessary to reduce the mix to a molten state varies, depending on the makeup of the mix itself, and will take anywhere from 2 to 5 hours. When the steel becomes fluid, it is usually good practice to "kill it," or, in other words, drive out the gases which would otherwise result in blow holes in the ingot. This process of "killing" usually takes from 20 minutes to one hour or longer.

Molds. The molds in general use are known as split angle molds. They are made in two pieces, held together by rings and wedges—one ring at the top and one at the bottom. The three essential qualities are long life, smooth finish and tight joints. If the inside finish is not smooth, the ingot will have a rough surface which may result in defects in the finished bar. If the joints are not tight, the hot metal will work through and form a fin on the ingot. This fin will have to be removed, which means added cost. If it is not removed, it will work into the steel and cause complications. The smaller molds have the bottoms cast with the sides. The larger molds, 7-inch and over, have no bottoms as a rule, the molds being set up on removable bottoms. Before the molds are used, the general practice is to smoke them with rosin, or some other heavy, greasy, smoke-making material. This prevents the ingots from sticking, and also makes a smoother surface. The molds should also be warmed before using.

Teeming or Pouring. Teeming is a very important feature and is not merely dumping the hot steel from the crucible into the mold in a haphazard way. In the first place, the stream must be steady; if it is stopped and then started again there will be a weak spot in the ingot. The

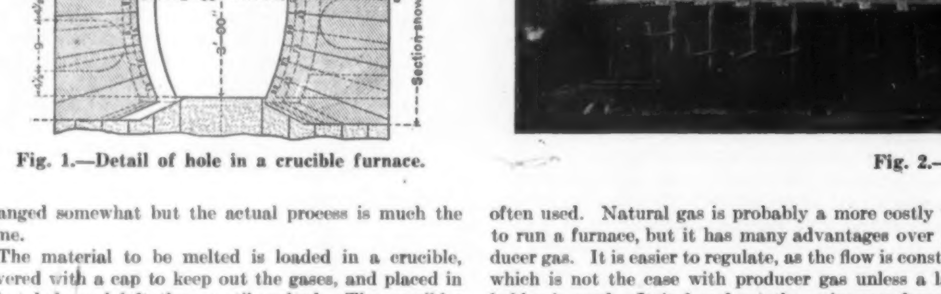


Fig. 3.—A 10-inch bar mill.

chilling of the metal first poured, however slight, will result in a non-homogeneous mass and the ingot when hammered will break at the point where the stopping of the stream occurred. The stream should never be allowed to strike the sides of the mold; if it does it will cut the mold and the result will be rough ingots, and in a heat or two put the mold out of commission; also the stream should be started as gently as possible. If it is teemed in without care the metal will splash against the sides of the mold, and cause the lower part of the ingot to be rough. This teeming is not as easy as it looks and it takes considerable practice to make a man an expert. The weight lifted is quite considerable; the crucible and the tongs weigh about 60 pounds and the steel about 100 pounds, a total of 160 pounds. When you remember that this weight has to be lifted and held steady so that the steel will flow from the crucible evenly and at a uniform rate, the difficulty of doing the work properly can be appreciated. It is not altogether a question of strength, it is knack. Some of our strongest furnace men never learned to teem properly. They had the strength but could not master the art. When a ladle is used, of course, the difficulty of teeming is done away with as the steel can be dumped into the ladle as fast as possible and the teeming is then done from the ladle itself. Both methods have their advantages. It is necessary that the molds be set up straight, or, in other words, plumb; if they are not the melter is more than likely to teem against the side, and also a mold out of plumb is apt to have a bad effect on the steel as it chills. Before the steel is poured out of the crucible, the dirt, which has risen to the top, should be removed. This is easily and quickly done by means of a steel rod known as a flux stick. The flux will adhere to it and can be removed without trouble.

Pipe. The worst enemy of the crucible steel melter is piping. Piping is caused by the sides of the ingot cooling faster than the center. The molten steel which comes in contact with the sides of the mold cools much faster than the center of the ingot. This cooling effect of the mold is felt for as great a distance, approximately, as the mold is thick. In other words, a mold 2½ inches thick will have a chilling effect on the hot steel for that depth, and the result is that the steel thus affected will separate from the rest and the pipe will form. Of course, this result is greatest at the top of the ingot for the reason that the tendency of the pipe to form lower down is offset by the metal from the upper part of the ingot filling in the space. The pipe usually continues as shown in the left hand section of Fig. 4, but often, especially if the teeming has been done too rapidly, the pipe appears as shown in the right hand section of Fig. 4. This is the most dangerous form of pipe, as it is not easy to detect and remains when the visible pipe has been removed. There is no cure for pipe after it gets into an ingot, as it cannot be welded out or worked out and will result in the splitting of the steel when hardened. The most general mode of treating pipe is to use hot tops. A hot top is a brick made of fire clay with a hole through it, the size of brick and hole depending on the size of the ingot cast. The method of handling hot tops is as follows: When the mold has been almost filled, the hot top is placed on top of the hot steel in the mold and the hole filled with the melted steel. This plug, as we may call it, settles into the pipe as it develops, and also has a tendency to keep the top of the ingot hot, and thus lessen the pipe. When the entire mass has cooled, the hot top is broken off and the top of the ingot appears as shown in the right hand ingot of Fig. 5, the hot top, which has been broken off, lying on top of the ingot. The result of teeming without a hot top is shown in the left hand ingot of Fig. 5. The hot top, however, does not prevent the formation of small cavities below the main portion of the pipe as shown in Fig. 4. It should be remembered that the hot top brick must be heated to as high a temperature as it will stand before being placed in the ingot. If this is not done, the cold brick will chill the steel and destroy the usefulness of the hot top.

To Decrease Pipe. A number of patent molds have been tried but all have been of indifferent success, and the added cost has worked against them. There is no doubt that the present style of mold aids piping, and all of us who are makers of high carbon steel are living in hopes that some day someone will discover a mold that will eliminate it, at least to a great extent. Some of the present molds, those for instance which are tapered with the large end up or those that have hot material packed around the top are merely adaptions of the hot top idea.

Topping. When the ingots are cold, they are removed from the mold and topped, that is, the top is broken off so that a clean fracture is obtained. This is not a laborious job and two trained toppers can top a large number of ingots during a day's work. A trained eye can tell from the fracture the carbon content of the ingot within 0.05 per cent. This is not as difficult as it may seem, and anyone with practice can become very efficient. The manganese, phosphorus, sulphur and silicon cannot be determined this way. Neither can the carbon of high speed steel be determined from the fracture.

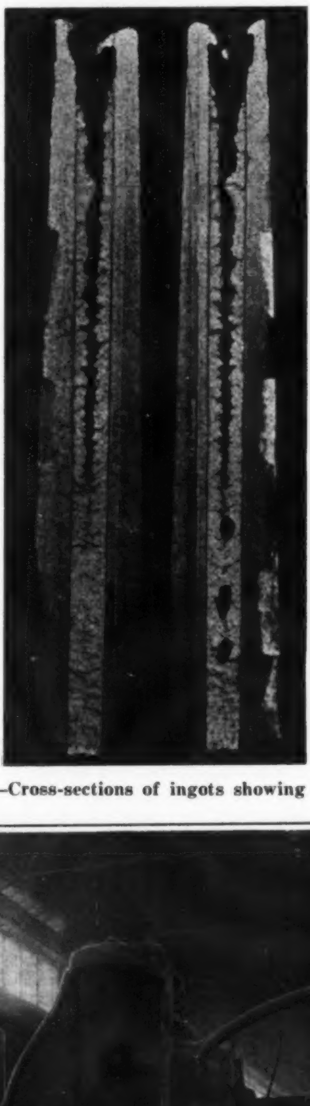


Fig. 4.—Cross-sections of ingots showing piping.

Working. The process of working the steel after it is made is of great importance and the old rule of thumb days are over. The heating of steel was guessed at and many a good piece of steel was ruined by a worker who inherited his trained eye from his grandfather. Luckily for the steel maker, the use of pyrometers is becoming more general every day and guessing at hardening temperatures is rarely done. No steel can be made fool proof, and no overheated steel can be made as good as it was before it was overheated. It can, if not too far gone, be restored partially, but that is all. High speed steel is as near fool proof as any, but even it can be harmed by too much fire. The result of overheating is interesting, and I have here some pieces of steel which show its effect. Later I will be glad to show them to anyone interested.

Rolling. Rolling, like hammering, must be carefully done, if good results are to be expected. The heating should be exact, not guessed at. If the heating is not made to conform to the carbon content of the steel, the results will not be satisfactory. Rolling crucible steel is not a tonnage proposition, it cannot be rushed out if good results are expected. It is unlike open hearth steel, where as a general thing "quantity" is the slogan. To illustrate the difference: In reducing a 3-inch square billet of open hearth to ½-inch round we would have, say 14 passes through a mill driven at high speed, and, at the finish, a bar of approximately 100 feet in length. With crucible steel, if a ½-inch round, we would have 21 passes through a mill driven much slower and a bar about 12 to 14 feet long, but the extra and slower work means a finished bar much closer to size, planished and free from scale. Fig. 3 shows a modern 10-inch bar mill for rolling high carbon crucible steel.

Hammers. Hammers are of two kinds, single leg and double leg. The single leg hammer has one advantage, the absence of one leg allowing the hammerman to work both across and lengthwise on his die, which is at times an advantage. This hammer, however, is more difficult to keep steady than the two leg hammer, as it has a tendency to spring with the blow of the ram and thus work loose on its foundation. The different size hammers and the size of the work usually done on them is as follows: A 500-pound hammer is capable of handling bars ¼-inch up to and including ¾-inch. Fig. 6 shows a 1,000-pound hammer which handles bars from ¾-inch to 1⅝-inch. A 2,000-pound hammer can work bars 1⅝-inch to 3-inch, and a three-ton hammer (see frontispiece), bars from 3 to 6-inch. Of course, smaller or larger sizes than those enumerated can be worked on the various hammers, but the general practice is within the limits given.

The 500-ton steam hydraulic press, shown in Fig. 7, will work high carbon ingots 16-inch square. The press has some advantages over a hammer. It is much easier on the workmen, as it is free from shock and jar, and for this same reason it does not cause deterioration of furnaces and foundations adjacent to it. It works the steel all the way through and gives it a density which a hammer does not. This is probably due to the fact that pressing the steel causes it to flow while the blow of the hammer

Fig. 5.—Ingots cast without and with hot tops.

Fig. 6.—A 1,000-pound hammer.

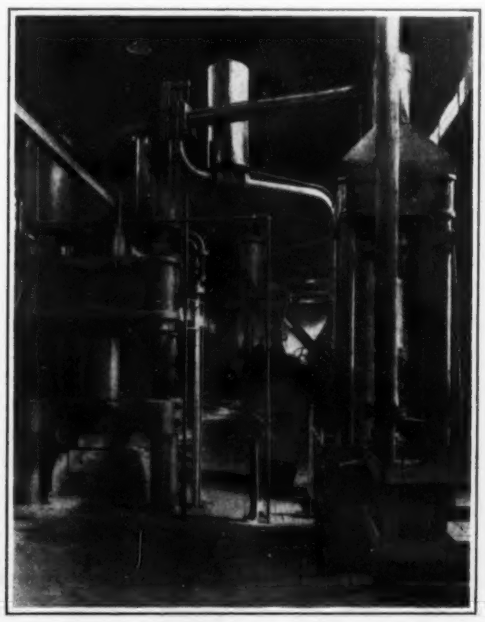


Fig. 7.—A 500-ton steam hydraulic forging press.

is merely local and is not sustained long enough to affect the steel to the center.

Hammering. Hammered steel, that is steel worked into shape under a hammer, must be very carefully handled if the best results are to be obtained. The bar to be hammered must not be overheated, if it is the coarse grain resulting will not respond to the refining influence of the hammer, but it must be soaked, or in other words heated through. The hammering must be done intelligently and the blows of the hammer regulated to correspond to the diminishing heat of the bar. It is also important that the work done should not be done under a hammer too heavy or too light for the work. A heavy hammer will rupture the steel and a hammer too light will necessitate too many blows and continued reheating. The weight of a hammer is, in shop parlance, governed by the weight of the ram, piston rod and piston head. For example, if the hammer is a 6-ton hammer then the rod, ram and head weigh 6 tons. The dies of the hammer are made of cast iron with a chilled surface, ground to a smooth finish. The proper grinding of dies is an art, for if the dies are improperly ground it is impossible to get good results, as the bar will be hard to hold and will jump at each blow of the hammer, the result being that the work will not be true to shape or size. Steel dies are sometimes used but they do not take the smooth surface and polish of the iron dies, and consequently will not planish the steel as well. The proper hammering of steel is the work of an expert. It cannot be learned in a day, and a man to be a good hammerman should have served an apprenticeship under a first-class worker. Although hammermen are paid by the ton the work is not rushed out, and quality not quantity should be the watchword. This does not mean that a man does not do a fair day's work. It is well known what a day's work is on any shape and size, but if the work is to be first-class as to finish and heat treatment, it cannot be rushed, but must be given its proper allowance of time.

Inspection. All crucible steel should be very thoroughly inspected for defects before shipping. It is much better for all concerned to keep your trouble at home, and if there is any doubt as to the soundness of a bar it should be scrapped. It will be cheaper in the end to do this than to take a chance. Inspectors should be given plenty of time and not hurried in their work. All bars should be topped and carefully examined for pipe. Usually pipe is easy to detect, but at times pipe shows in the form of a bright spot, no larger than a pin point, known as a "star." It does not indicate the size of the pipe further in the bar, and must be followed until no trace of pipe can be found. If the surfaces of the finished bars show seams they should be filed out if not too deep. If allowed to remain, they will cause trouble, especially in a cutting tool, as they will cause cracks when the steel is hardened. If the seams are too deep to file out easily, the bar should be scrapped.

Carbon and Alloy Steels. The usefulness of alloy steels was not generally known until a few years ago. Mr. Robert Mushet, who was the pioneer manufacturer of self-hardening or air-hardening steel, made his discovery in 1868. This steel differed from the ordinary or straight carbon steel in that it contained tungsten and that it would harden if heated and then allowed to cool in the air. This Mushet steel had the field to itself until Mr. F. W. Taylor and Mr. Maunsel White, of the Bethlehem Steel Company, discovered high speed steel. This discovery was the result of about 25 years of hard work. About 50,000 experiments were made and recorded at an approximate cost of $200,000. It was unfortunate that these gentlemen did not receive the reward their labors surely entitled them to. Their application for a patent was denied and there was a general rush of steel manufacturers into the high speed market, and to-day there are over 100 brands of high speed steel. The Taylor-White steel was a tungsten steel with the addition of vanadium. These "tungsten-vanadium" high speed steels had the market to themselves until Becker introduced cobalt steel. This steel is a tungsten-vanadium-cobalt steel, which has some valuable properties. Tungsten interested the scientific world some years before Mushet demonstrated its value. We find that in 1783 tungsten was found in the metallic state by D'Elbingar, who presented a memoir on the subject to the Academy of Science at Toulouse. The Duc de Luynes in 1844 published a memoir on the manufacture of cast and damasked steel wherein he pointed out that tungsten appeared in 8 of 9 analyses given of oriental damasked steel. At the Congress of Miners and Smelters, held in Vienna in 1858, many specimens of tungsten steels were shown. At that time, there were many advocates of the good results of tungsten and also many who could see in it no value.

Do not confuse tool steel with crucible steel. This is a very common error and one which should be avoided. Cast steel is also misleading inasmuch as it often deceives the user. Many of the cheaper grades of tools are made of open hearth steel, which is also a cast steel, so it is seen that tool steel and cast steel do not necessarily mean crucible steel. Another error which is frequently run into is the supposition that the grade of steel depends on its carbon content. This is a mistake and the ordinary grades, I mean by ordinary, straight carbon steel, or in other words non-alloy steel, can be furnished in any reasonable carbon. Another misapprehension under which many users of steel labor is that the grades of steel can be established by analysis. This is, of course, not the case. I have seen many analyses of open hearth steel which were better than the ordinary grades of crucible steel. If the chemical analysis of steel was all that was necessary to establish its worth, the open hearth would have put the crucible out of business long ago.

Edible Fungi*

By W. A. Murrill

The use of mushrooms in this country is as yet very limited, and every season an immense quantity of nutritious, digestible, and palatable food goes to waste in our fields and forests which would be utilized in China and many other parts of the Old World. The reason for this is ignorance and fear; lack of knowledge regarding the edible kinds, and a very definite impression that some of them, or most of them, are dangerous.

All knowledge regarding the edible and poisonous properties of mushrooms is based on experiments, either intentional or unintentional. The only safe rule is to confine oneself to known edible forms until others are proven harmless. If one is a beginner, he is like an explorer in a new country with an abundance of attractive fruit near at hand, which may be good or may be rank poison; he cannot tell without trying it, unless some native, who has learned from his own and others' experience, shares his knowledge with him.

The majority of fleshy fungi are edible. A certain number are bitter, or peppery, or slightly poisonous, or otherwise objectionable, but not deadly. Their digestibility often depends on the way they are prepared and cooked, and on the peculiarities of the individual who eats them. A few are deadly poisonous. Two species, *Venenarius phalloides* and *Venenarius muscarius*, are responsible for most of the deaths from mushroom eating the world over. If these two were thoroughly known and avoided in the vicinity of New York city, there would probably be no fatalities here from mushroom-eating for the next ten years.

My advice to beginners is to confine themselves at first to groups that contain no poisonous species so far as known, or to certain species that cannot be easily confused with harmful ones.

EDIBLE FUNGI FOR BEGINNERS.

Common mushroom, morel, chantarelle, beefsteak, and sulphur-colored polypore.

Shaggy-mane, common inkcap, and glistening inkcap.

All puffballs, provided they are white, tender, and homogeneous within.

All coral-fungi, if they are fresh, crisp, tender, and have no bad odor nor bad taste.

The oyster mushroom and its near relatives. These are large, with white gills and short stems, and grow on dead wood above ground.

After considerable study and experience, more difficult distinctions may be made and other groups taken up.

SOME CRITICAL EDIBLE SPECIES.

Polypores that are sufficiently tender, avoiding certain Boleti and *Fomes Laricis*.

Boleti that have been tested and found edible, avoiding *Suillellus luridus*, *Ceriomyces miniato-olivaceus*, and *Tylopilus felleus* in particular, or all species with red tube-mouths and bitter or peppery taste, and species that turn blue quickly when handled.

Species of *Russula* and *Lactaria* with pleasant odor and flavor, avoiding such species as *L. rufa*, *L. torminosa*, *R. foetens*, and *R. emetica*.

Several species of *Lepiota*, avoiding *L. Morgani*, with green spores, and species of *Venenarius*.

Marasmius oreades must not be confused with *M. urens*, nor with *Inocybe infida*.

Clitocybe, *Tricholoma*, and *Collybia* are usually edible; avoid *Clitocybe illudens*. *Vaginata* too closely resembles *Venenarius*.

Before attempting to use mushrooms at all for food, one should become acquainted with the chief poisonous species, if possible, by consulting any one of several books on mushrooms to be found in the public libraries. The deadly poisonous species are included in the genus *Venenarius*, formerly known as *Amanita*. *Venenarius cothurnatus* is much more common farther south, and *V. solitarius* can hardly be called deadly.

THE CHIEF POISONOUS SPECIES.

Venenarius phalloides, *V. muscarius*, *V. cothurnatus*, and *V. solitarius*.

Clitocybe illudens.

Inocybe infida.

Panus stypticus.

Chlorophyllum Molybdites (*Lepiota Morgani*).

Russula and *Lactaria*, about ten species.

Rosy-spored species, a few.

Several of the phalloids, probably.

Several species not yet tested, doubtless.

Note that no brown-spored, purplish-brown-spored, nor black-spored species are listed above, but not all have been tested.

Nearly two hundred water-color drawings of local edible and poisonous mushrooms have recently been installed in the public museum of the New York Botanical Garden. These are not accompanied by descriptions, nor are the edible species designated, but the student of fungi will have no difficulty in recognizing most of the common local species from these drawings alone.

PREPARING AND COOKING MUSHROOMS.

Reject old specimens or those infected with insects, cut off the stems except in rare cases where they are unusually tender, peel a few kinds that seem to require it, wash quickly in cold water, drain and keep in a cool place until ready to cook. As a rule, mushrooms cannot be kept very long in a fresh condition, and this is particularly true of certain very desirable species. When more are collected than can be used at once, it is best to boil them ten minutes, drain, keep in a cool place, and finish the cooking next day as desired. If allowed to stand in water, the flavor is impaired; also, peeling may remove some of the best flavored parts.

Detailed directions for cooking mushrooms are given in most of the books. The most practical and successful methods resolve themselves into broiling, baking, and stewing. In the first, which I prefer to all other methods, the mushrooms are cooked thoroughly but as quickly as possible on both sides over a hot fire; seasoned with pepper, salt, butter, and perhaps small bits of toasted bacon; and served hot on toast. To bake mushrooms, line the pan with toast, add the specimens, season, pour in half a cup of cream, cover closely, and bake rather slowly for fifteen minutes or more, according to quality. In stewing, the mushrooms are boiled in water until thoroughly cooked, then seasoned, thickened, and served on toast. This last method is often used for the tougher or poorer varieties.

* Abstract of a lecture delivered at the New York Botanical Garden and published in the *Journal* of that institution.

The Application of Jets for Mixing Purposes

By Oskar Nagel

Jet appliances consist principally of two or more nozzles of increasing diameter and so connected with each other that the jet of gas or liquid passes from a narrow to a wider nozzle, whereby a vacuum is created, and the material that is to be moved is transported by the jet, which acts as motor. Such apparatus is easily handled, has no moving parts, and requires little repair, so that its low efficiency as a motor is not a drawback when used only for intermittent work. The injector, however, occupies an exceptional position, since the heat energy of the steam is recovered by being returned to the boiler.

The low efficiency of jet apparatus as "transporters" is caused by the fact that about 75 per cent of the energy of the jet is consumed in the whirl, which is formed at the transit of the jet from one nozzle to the other. This whirl effects so intimate a mixture of the motor jet with the medium to be moved, that it is a matter of surprise that jet appliances have not been used as "mixers," rather than "transporters."

The chemical industry rather lacks mixing appliances for gases and liquids, and the jet apparatus is well worth consideration for certain operations. One of these is the chamber process of manufacturing sulphuric acid, which requires chambers of very large dimensions in order to mix the gases thoroughly during their passage. Were it possible to introduce the gases as a perfect mixture into the chamber, its dimensions could be much reduced.

While this apparatus has been recommended for moving the gases through the chamber system, its effect as a mixing machine has been entirely overlooked.

In order to use the jet blower as a mixer it must be connected with the system in a different way from what it would be if used for the transportation of gases. It is not enough to lead steam jets through the wall of the chamber, nor to install a steam jet blower without housing into the pipe coming from the Glover. With such an installation only a part of the gases would get incorporated into the whirl, and a partial mixture only would take place. In order to effect a perfect mixture, the whole of the gas must be forced through the nozzles. This is accomplished by tightly connecting the gas pipe of the system, preferably that part which leads from the Glover to the first chamber, with the inlet opening of the steam jet blower.

In order to prevent the gases from entering the chamber at too high a temperature, it is advisable to enlarge

the lower part of the casing of the steam jet blower. In the space so gained a tubular cooler is provided (for either air or water), in order to cool the gases to 65 deg. Cent. The gases travel from the Glover through the jet blower to a small chamber, and from here through two reaction towers to the Gay Lussac.

In order to increase the mixing effect of the apparatus as much as possible and to reduce the "motor-effect" of the jet, it is best to have the gas delivered at the inlet opening of the apparatus at atmospheric pressure, so that the function of the jet blower is exclusively to mix the gases, the latter being delivered at the outlet of the apparatus at atmospheric pressure. By this mode of working the efficiency of the jet as a mixing machine reaches a maximum, at which 20 cubic meters of air is drawn through the nozzle by means of 1 kilogramme of steam of 2 atmospheres.

It is clear from these figures that in the chamber process less steam is needed for effecting the mixture of the gases than is required for the reaction leading to the formation of sulphuric acid. The plus of steam necessary for the formation of the acid can either also be sent through the jet apparatus (in which case the apparatus would need to be correspondingly larger), the energy of the plus steam being utilized for the increase of the draught in the chambers, or it may be injected directly into the chamber by means of spray nozzles.

Preliminary experiments show that for a plant in which 3,150 kilogrammes of sulphur are burnt per day, a chamber of a capacity of 850 cubic meters may be sufficient, if after the chamber two reaction towers are provided. The first tower should contain about 20 layers of 20 plates, the second about 30 layers of 12 plates each.

While we are able to utilize the steam jet as a mixer in the chamber process, we can with the same convenience apply the liquid jet in the contact process. In this case sulphuric acid is used as "motor" liquid, while trioxide is the material to be drawn into the whirl. By using a water jet condenser very satisfactory results are obtained in this process of absorption, and also in other processes of similar nature.

An effect in many respects similar to that of jet apparatus is obtained by spray nozzles, since the spray, in forming a hollow cone, creates a vacuum in the direction of its motion. On the other hand, the spray is also a mixing machine and adapted to effect absorption of large volumes of gases with small volumes of liquids. For this purpose the spray nozzle must be inclosed in a suitable cover in order to force every particle of the gas through the hollow cone of the sprayed liquid.—*Journal of the Society of Chemical Industry.*

A Congress of Scientific Kite-flying was held at Boulogne-sur-Mer, France, May 31st and June 1st, 1914, under the auspices of the Ligue Française du Cerf-Volant.

The Rockefeller Institute for Medical Research

A STATEMENT has been given out from the Rockefeller Institute for Medical Research to the effect that in order that further opportunities may be afforded for the more complete investigation of the nature and causes of human disease and methods of its prevention and treatment, Mr. John D. Rockefeller has just donated $2,550,000 to the Rockefeller Institute for Medical Research.

Of the sum just donated a part will be utilized to purchase additional land in New York city so that the institute will have acquired the entire tract where its buildings are now located, between Sixty-fourth and Sixty-seventh streets on Avenue A, extending through to East River—about four acres. The remainder will be used to erect and equip additional laboratories, buildings, and plant, and to insure the proper maintenance and conduct of the extended work.

This gift of $2,550,000 is in addition to a special fund of $1,000,000 which Mr. Rockefeller has provided in order that the institute may establish a Department of Animal Pathology. Dr. Theobald Smith, now professor of comparative pathology in Harvard Medical School, is to become director of the new department.

It will be the purpose of this branch of the institute's work to give special attention to the study of maladies such as hog cholera, foot and mouth disease, and diseases of poultry, which are of such immediate and practical concern to farmers, and the elimination of which is so important. This will be the first enterprise of this kind upon an adequate basis to be established in this country. The results of its work should eventually be of great value in improving the health of cattle and other farm animals.

Mr. Rockefeller's previous gifts to the institute had amounted to practically $9,000,000, exclusive of real estate in New York city, so that the endowment of the institute will now approximate $12,500,000.

The Rockefeller Institute will, with the new gift, now become the most amply endowed institution for medical research in the world. In 1902, when the institute was founded, there was not a single undertaking of the kind in this country. England had the Lister Institute, Germany the Institute for Infectious Diseases, France the Pasteur Institute and Russia the Royal Military Institute at St. Petersburg. Since 1902 a number of other research laboratories have been established in this country, including several in Chicago.

In addition to the laboratories there is connected with the institute a hospital with every improved facility for the treatment of patients afflicted with diseases at the time under special investigation. For the treatment and study of contagious diseases—a most important phase of the institute work—there is a separate building with isolated rooms.

The aims of the Rockefeller Institute and the lines along which its future work—upon an even more comprehensive basis—will be conducted, are indicated by some of its practical achievements already accomplished, such as the serum treatment of epidemic meningitis; the discovery of the cause and mode of infection of infantile paralysis, the surgery of blood vessels, through which blood transfusion has become a daily life-saving expedient; the safer method of administering anesthetics by intratracheal insufflation; the skin or luetic reaction and the cultivation of the parasite of rabies.

The scope of the work of the institute will be indicated by a list of the several special scientific departments which it maintains. It includes pathology, bacteriology, protozoology, biological chemistry, physiology and pharmacology, experimental biology, and animal pathology, besides the special hospital.—*Science.*

Iceplant as a Food

MESEMBRYANTHEMUM CRYSTALLINUM is the scientific name of the singular crystal-covered, white-flowered iceplant, so indispensable in a rockery. *Mesembryanthemum tricolor*, with rose-colored flowers, is the dew-plant. *Mesembryanthemum variegatum* has variegated green and white foliage, with sometimes yellow and sometimes pink flowers.

All kinds are propagated by both seeds and cuttings.

In France the iceplant is cultivated for greens, and cooked like spinach. In England it is valued for garnishing, as we use parsley. The troublesome weed pusley is often used for greens in this country, and is a near relative of the iceplant.

In Africa *Mesembryanthemum edule*, the fig marigold, is highly prized by the Hottentots, who eat the figlike fruits. In Palestine the Arabs make a bread claimed to be more nutritious than wheat, from the seeds of *Mesembryanthemum forskahci*. This plant is of great value for forage, and the seed pods will not open with heat, so the seed gathering is extended through several months. The seed is soaked in water to open the pods, and when stirred, the pods float on top of the water and the seeds sink to the bottom. They are next dried in the sun and ground into flour and baked in cakes sweetened with a molasses made by boiling the seeds of *Juniperus Phoenicea* and straining the liquid.

The United States Department of Agriculture has secured a considerable quantity of the seed, which will surely be of great value as a forage plant, sheep and cattle eating it greedily, the succulent foliage answering for both food and drink. Many lives would have been saved if iceplant of different species had been found growing in our arid southwestern States; and stranded cattlemen and prospectors of the future will have reason to thank Uncle Sam for providing this manna on the desert.

The Safe Operation of Pleasure Cars*

Suggestions That No Automobile Owner Can Afford to Ignore

LESS than a quarter of a century ago a person would have been considered visionary and unpractical, to say the least, if he had predicted that a self-propelled vehicle would soon be invented that would travel over the public highways at speeds as high as a mile a minute, and with power sufficient to enable it to climb the steepest hills with ease. To-day a performance of this kind is commonplace, and no special comment is excited by published accounts of automobile races in which speeds of 80, 90, or even 100 miles an hour are attained. When the automobile reached the stage of mechanical perfection that permitted it to travel twenty-five miles or so, without stopping for repairs, it was considered marvelously efficient; but at the present time delays on the road due to mechanical troubles are rare, and the tourist may often continue his journey day after day, without giving any special attention to his machine, beyond providing fuel and supplying the necessary oil and grease for lubrication.

The evolution of the automobile, from the slow, uncomfortable, cumbersome, and mechanically imperfect conveyance of the past, to the swift, dependable, luxurious car of to-day, has been attended by a correspondingly increasing amount of danger, not only to the operator of the car and his passengers, but also to the public at large. The number of accidental deaths in cities has largely increased since the advent of the automobile, and in country towns and on all public highways the danger of being struck by a swiftly moving car, in charge of an inexperienced or careless driver, is always present. Even the careful driver may not entirely escape accidents, because many injuries are direct consequences of carelessness or inattention on the part of pedestrians and drivers of other vehicles. Some of these accidents may be avoided by constant watchfulness on the part of the automobile operator, but in other cases no amount of care will enable him to escape them.

There is no doubt that the blame for many accidents is unjustly laid upon the shoulders of the automobilist, and it is equally true that the neglect of some simple precaution on his part has often led to serious injuries, or even to loss of life. The following suggestions are given in the hope that the risks to users of the public streets and highways may be lessened, and that the operation of automobiles may also be made safer for drivers and passengers.

One of the prime requisites in the safe operation of an automobile is a thorough and instinctive knowledge, on the part of the driver, of the uses of the various levers and pedals that control the movements of the car. The seasoned driver does not stop to think what motions must be made to bring the car to a sudden stop in case of an emergency. When an accident seems imminent he instinctively throws out the clutch and applies the brake, in a mere fraction of the time that would be required if he were obliged to think out each motion in advance. No person should attempt to operate a car in crowded traffic until he has acquired something of this manipulative instinct, and a beginner should not trust himself to drive a car under any circumstances, until he has received adequate instructions from a skilled and experienced operator.

Before leaving the garage the car should be looked over carefully to see that everything about it is in a safe condition. The steering gear and the brakes should receive particular attention in this respect, and loose nuts and other defective parts should be attended to in a thorough manner. The tires should also be examined for weak spots that may blow out while on the road, and tires showing such imperfections should be changed, if necessary, before starting out. Many accidents might be averted by paying proper attention to these points, and by making necessary repairs as soon as the defects are discovered, instead of waiting until a more convenient time.

Although many of the more recently built automobiles are equipped with self-starting devices, the great majority of automobile engines are probably started by means of a crank-handle. The arms, hands, and wrists of the operators are often broken, sprained, or badly bruised by "kick-backs" from these starting handles. Safety handles can be had, which practically eliminate these dangers; but for some reason or other they do not appear to be widely used. Before attempting to crank the engine, the spark should be retarded, to prevent too early ignition. The operator should grasp the crank with his *left* hand, placing his thumb along the length of the handle rather than around it, and *pull upward* on it. When starting a large engine with high compression, it is quite common for a driver to "rock" the handle, to obtain the advantage of the slight momentum given to the flywheel in this way, so that the engine may be more easily turned over the compression stroke; but this is a dangerous practice, and it should be avoided as far as possible. A striking example recently came to our notice, of the utter disregard for safety that is occasionally displayed by drivers

* Reproduced from *The Travelers Standard.*

in starting their cars. In this case the operator placed the starting handle in a horizontal position, and then *stood* upon it in order to turn the engine over.

Before attempting to start the engine, care should be taken to see that the clutch has been withdrawn, or that the gear-shift lever is in the neutral position. It is also important to make sure that the brakes are set up tightly if the car is standing on an incline, because if this is neglected the vibration of the engine, after it has been started, may set the car in motion.

When operating a car on the road the driver should listen for unusual sounds about the machine, and if such are noted he should make an immediate investigation to determine the cause. Any marked variation from the usual action of the car also indicates that something is out of order, and prompt measures should be taken to discover the source of the trouble, and to apply the proper remedy.

Attention has been called to the danger of allowing inexperienced persons to operate cars on crowded city streets, where the conditions are entirely different from those that are met with on public highways outside of the city limits, or in public parks. It is necessary, of course, to obey all local traffic regulations, and the driver should faithfully adhere to these regulations, even in the absence of traffic officers. When approaching a stationary street car that may be receiving or discharging passengers, the driver should proceed with extreme caution, and it is safer to bring the automobile to a standstill at a distance of at least eight or ten feet from the street car, and to wait until the passengers have reached a place of safety. Children, and even grown persons, often become confused and suddenly move in directions totally unexpected and unforeseen by the automobile operator, and he should be on his guard against behavior of this kind. The driver, when running the car near the curb, should always be on the watch for persons who may step from the sidewalk into the street without looking about them, or giving indications of their intentions in advance. The most careful person is likely to do this at times, through absentmindedness or preoccupation, and it is only by exercising constant vigilance that the automobilist can avoid accidents due to this cause.

When a traffic officer holds up a car at a street crossing, or at any other point, it is a good plan to shift the gear lever, *immediately*, to the proper position for starting. Delay in doing this is dangerous, because circumstances may arise that will necessitate a quick "getaway" to avoid an accident. Ordinarily, however, the automobile should be started cautiously after a traffic officer has given the signal to proceed at a street crossing, because some person is usually present who is impatient to cross, and who will try to do so in front of the car. The right of way should always be conceded, without hesitation, to fire-engines and other fire-department apparatus, and if it is practicable to do so, the automobile driver should draw his car up beside the curb until all the apparatus has passed.

In many cities signs are now posted near schools, warning automobilists against fast driving at these points. These warnings and all other similar cautions should be faithfully heeded, and the necessary precautions should be taken to prevent accidents. It is best to avoid the neighborhood of schools altogether, so far as this is practicable.

A suitable warning should be given by a motion of the hand, or in some other manner, before backing the car, turning it around, crossing from one side of the street to the other, turning into a side street, or coming to a stop. If he considers it safe to do so, the driver should glance backward for a moment to see that his signal has been observed by the driver of any vehicle that may be following him, and he should never change his direction until he is satisfied that he has a clear course. Where streets intersect, precedence should be given, in every case, to a car that is following the main avenue of traffic, because any other course may lead to confusion and disaster. At corners where the view is obstructed by buildings, or by high bushes or trees, or in any other way, the speed of the car should be reduced and the horn should be sounded.

Accidents frequently result from the omission of proper precautions when leaving the car unattended temporarily. The switch plug should be removed, the gear-shift lever should be placed in the neutral position, and the brakes should be set up tightly. If the car is standing on an incline it is advisable, in addition, to turn the wheels so that one of them will rest against the curb. If this is done there will be no danger of the car running away in case some mischievous or meddlesome person should release the brakes.

A recent accident, that occurred while one car was towing another one, is instructive and may serve to direct attention to a source of danger that is often overlooked. A man who was waiting to cross the street attempted to pass between the two cars, not noticing the rope that connected them; and before he could step back out of the way he was struck and injured by the car that was being towed. When towing, the driver of the leading car should exercise great caution in every respect. He should drive slowly and carefully, and should make it as evident as possible, to persons who are waiting to cross the street, that another car is attached to the one he is driving.

In addition to the suggestions that have already been given, and that are applicable more especially to the operation of automobiles in the cities and larger towns, there are others that should be mentioned in connection with the operation of cars on the public highways outside of the more thickly populated districts. Excessive speed is one of the chief causes of accidents, and many drivers have a mania for traveling at a high rate, even when there is no definite object in doing so, and when conditions make the practice distinctly hazardous. Except in special emergencies the speed of the car should be regulated to suit the condition of the roads and the density of the traffic. Much depends also upon the skill and experience of the driver, and the automobile operator who is constantly mindful of his own safety and of that of his passengers, will not willingly take chances of any kind. Laws limiting the speed of automobiles have been enacted in many states, and it is safe to say that if the legal speeds were never exceeded the number of accidents would be greatly reduced. Racing on the highways with other automobiles, or with railroad trains or trolley cars, is a dangerous practice, in which no driver should ever indulge.

Fig. 1.—The result of skidding.

Safety requires that drivers should always keep on the right-hand side of the road, except when passing other vehicles that are going in the same direction. In such cases the vehicle that is coming up from the rear should always pass to the *left* of the overtaken vehicle. There should be no exceptions to this rule, as any other course leads to confusion and may cause accidents. Some authorities maintain that an exception should be made when passing a trolley car, a furniture van, or any other large, high wagon or load, the size of which prevents the driver of the automobile from seeing ahead on the road; and traffic officers in some cities require automobiles to turn to the *right* in overtaking vehicles of this nature. Unless local traffic regulations require turning to the right, however, we are of the opinion that the rule to turn to the left in overtaking other vehicles should still be followed, even in these cases, although it is then especially important not to turn out too quickly or abruptly. A long sweeping turn should be made, at reduced speed, and preferably at some considerable distance behind the vehicle that is to be passed; and the driver should look sharply ahead in all such cases, so that he will see, at the earliest moment possible, any teams, automobiles, trolley cars, or pedestrians that may be in his course. When about to pass a vehicle from behind, an adequate warning signal should be given, and the speed of the car should be reduced whenever such reduction will diminish the likelihood of accident. A driver should never attempt to exercise his skill by turning in as closely as possible in front of the vehicle that has just been passed, but should allow plenty of room to avoid striking it. Similar rules should be observed when meeting a vehicle coming from the opposite direction, and plenty of room should be allowed in every case. Operators of large cars sometimes "hog the road" (to use an expressive though inelegant phrase), and fail to show proper consideration to the drivers of small cars and of horse-drawn vehicles. Conduct of this kind causes ill-feeling, and in many cases it is also extremely dangerous.

Fig. 2.—The result of breaking a steering knuckle.

When making repairs on the road, care should be taken to move the car as far as possible to one side of the traveled part of the thoroughfare, and no tools, spare tires, nor other material should be left lying in the path of other automobiles. When changing tires or doing other work upon the car while on the road, or when getting out of the car for any purpose, a constant watch should also be kept, to avoid being struck by passing vehicles.

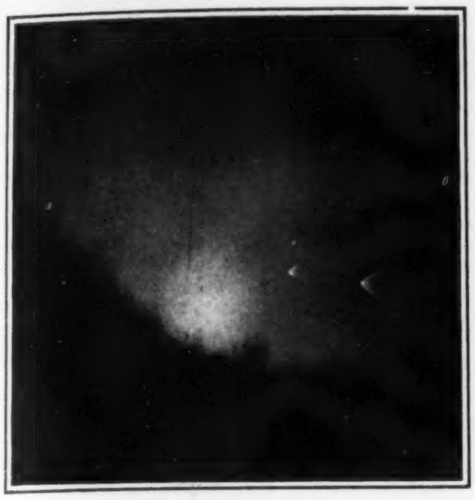
When approaching a frightened horse the speed of the car should be reduced, and if necessary the car should be brought to a standstill, and the engine stopped. Similar care should be exercised when approaching herds of cattle, the car being kept under perfect control so that it can be stopped very quickly. The car should also be kept under full control when descending long or steep grades, and if any doubt exists in the mind of the driver as to the adequacy of his brakes, he should shift his gear lever to low speed, cut off the ignition current, let in his clutch, and thus allow the engine to act as a brake. If it seems likely that the brakes are becoming overheated when descending a long incline, the car should be stopped and the wheels be securely blocked, until the brakes have become cool again. If the brake bands and drums are at a high temperature, they should not be chilled by pouring water upon them, but should be allowed to cool off gradually.

If the engine "dies" (or becomes stalled) while ascending a hill, and the brakes fail to hold the car, the engine may be used as a brake by shifting the gear lever to the reverse position, cutting off the ignition current, and letting in the clutch. This must be done, however, before the car has attained any great speed, because otherwise the gears are likely to be stripped when the clutch is let in, and the danger will be increased. If an emergency of this kind arises the driver should keep as cool as possible, and should remember that the safety of his passengers depends entirely upon his skill, and upon prompt action on his part.

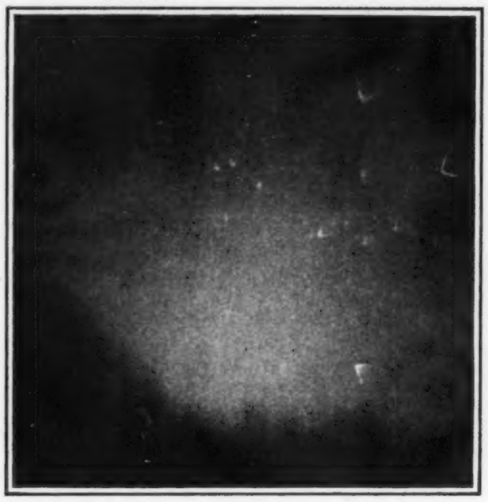
Special care should be taken when driving at night, and in fog, rain, and snow squalls. If the glare of the lights on an approaching car interferes with the view of the road ahead, the car should be slowed down or brought to a complete stop. When approaching unguarded railroad crossings the car should be stopped, if the view is obstructed in either direction, and one of the passengers should be sent ahead to see that all is safe. If the engine should become stalled while crossing a railroad track, the passengers should immediately get out of the car and go to a safe place, meanwhile keeping watch in both directions, in order to warn the driver of approaching trains.

A few general suggestions that are applicable in all cases may be given in closing this article. Intoxicated persons should never be permitted to operate automobiles under any circumstances, even though the intoxication is but slight. Drivers should be on the watch for children who may be "stealing rides" on teams, or who are playing in the streets or on the sidewalks. The operator should at all times keep his eyes on the road ahead; he should not turn around to talk with persons on the rear seat, and he should never remove his hands from the steering wheel while the car is in motion. If the steering apparatus or brakes are known to be defective or out of order, the car should not be operated until repairs have been made. The lights, and particularly the rear light, should be kept brightly burning when driving at night; chains should be used on the wheels during rainy weather and when the roads are freshly oiled or are covered with snow; attention should be given to the proper lubrication of all parts; and the lights of the car should never be left burning while filling the gasoline tanks. Water will not extinguish a gasoline fire, and fires of this kind should therefore be fought with sand, earth or generous applications of sawdust, or with special chemical fire extinguishers.

The most important caution that can be given in relation to automobile operation, is to refrain from taking any known and recognized chances, whatsoever. The temptation is great to utilize to the full the enormous power that the automobile engine can exert, and the exhilarating effects of the high speed that may be attained in modern pleasure cars lead many drivers and owners to the most unwise extremes. Safety should be the first consideration in every case, and recklessness and foolhardiness on the part of automobile operators should be discouraged in every possible way.



Taken February 13th, 1901. Z 84. Clear aperture of lens 0.9 inch. Ratio 1:2. Focal length 1.8 inch. Exposure 7 hours 35 minutes — 7 hours 50 minutes = 15 minutes.



Taken February 13th, 1901. Z 85. Clear aperture of lens 0.9 inch. Ratio 1:2. Focal length 1.8 inch. Made by Alvan Clark, Cambridge Port, U. S. A.

Taken March 9th, 1901. Z 114. Clear aperture of lens 0.9 inch. Ratio 1:2. Focal length 1.8 inch. Made by Alvan Clark, Cambridge Port, U. S. A.

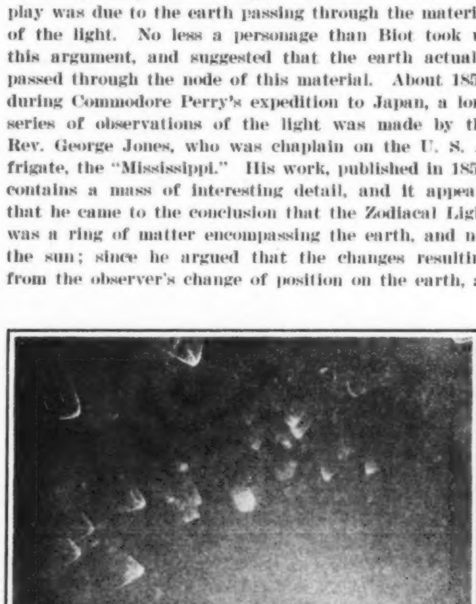
The Zodiacal Light*

Evidence of Cosmic Dust Scattered Through Space

By the Rev. J. T. W. Claridge, M.A., F.R.A.S.

Photographs by A. E. Douglass, Flagstaff, Ariz.

What a remarkable phenomenon is that luminous object in the heavens occasionally seen at this season of the year by the naked eye, and yet incapable of distinct observation by the telescope! Such is the Zodiacal Light. It may be taken for granted that of all the forms which light takes, perhaps this can safely be described as the most delicately beautiful. More milky than the Milky Way, more translucent than the filmy nebulæ, and not altogether unlike the Aurora Borealis, with which it has sometimes been associated, it seems like the concentrated essence of twilight, shooting up into the sky as the sun goes down about the time of the vernal equinox, or preceding the sun as it rises in the autumn. At sunset in March, April, or May, if the atmospheric conditions be favorable, there can be seen a bright tract of the heavens which may be defined as a kind of elongated triangular pyramid or spire resting upon that part of the horizon beneath which the sun has set. Away from all glare of gas light or electric light, it may be discerned without much difficulty. Of course, there must be no moonlight, nor even the brightness of Jupiter or Venus, to interfere with its view. With regard to its position, for all practical purposes we may say that its axis coincides with the plane of the ecliptic or of the sun's equator. This definition, though fairly correct for high latitudes, does not accurately represent it as seen in the tropical regions, where its light is often very conspicuous, and may be seen throughout the year. To an observer in England the cone of light leans somewhat to the left, and lies along the line of the Zodiac. Hence the origin of the name "Zodiacal Light," given by Cassini in 1683. In concord with other celestial objects, it sinks in the west by virtue of the earth's rotation, thereby showing that its existence is external to the earth's atmosphere. Pliny, who flourished in the first century of the Christian era, in his writings seems to allude to this phenomenon under the name of "*trabes lucis*" ("beams of light"), and the renowned Kepler (1571-1630) came to the conclusion that the Zodiacal Light was an atmosphere of the sun. Later on, Cassini, who died in 1712, after several years of observations at Nice, remarked that the northern edge of the light leaned more and more from its ecliptical axis during the months of March and April, when the solar equator was increasing its inclination to the ecliptic, and consequently he concluded that it was a solar appendage. He further described it as having a lenticular shape; that its diameter in June was equal to that of the sun, but much larger in March. In 1731 Mairan spoke of it as a solar reflection, having the form of a flattened spheroid. Humboldt, in his striking work "Cosmos," gives some very interesting notes on the subject, not the least being, in his opinion, that the ancients were unacquainted with it, notwithstanding the clearness of the eastern sky. From his own observations in South Africa, he describes the light as of a "blazing" character at one time, and as an "exquisitely delicate and ethereal object" at another. The remarkable meteoric shower of 1833 caused a goodly number of speculations as to the composition of the light. By many it was thought at the time that the display was due to the earth passing through the material of the light. No less a personage than Biot took up this argument, and suggested that the earth actually passed through the node of this material. About 1852, during Commodore Perry's expedition to Japan, a long series of observations of the light was made by the Rev. George Jones, who was chaplain on the U. S. A. frigate, the "Mississippi." His work, published in 1856, contains a mass of interesting detail, and it appears that he came to the conclusion that the Zodiacal Light was a ring of matter encompassing the earth, and not the sun; since he argued that the changes resulting from the observer's change of position on the earth, as well as the alteration in position caused by the earth's rotation, seemed to him much greater than could be explained if the ring were not relatively near the earth. But surely to this it may be replied that no ring surrounding the earth can in any way satisfactorily explain the phenomenon of the Zodiacal Light. But, assuming it were so, it is quite evident that, at a distance so moderate that a traveler in the tropical regions could recognize the change of position of the light as he passed from the north to the south side of the Equator, it would be invisible from places in high latitudes. In 1864 M. Chacornac observed the light in Paris and Lyons, and said that it was of sufficient power to obliterate stars of the twelfth and thirteenth magnitudes, and covers with a yellowish-red veil the region of the sky on which it is projected. The theory which connects it more immediately with the sun has stood the test of more recent observations. The faint yet exquisite, luster has so far proved amenable to optical tests that the spectrum has revealed its identity with the solar radiance. The light is sunlight, and only differs from the latter in its intensity. It is due to reflection, and, whatever may be the nature of the material which thus reflects the solar light, we may at once dismiss the idea that the Zodiacal Light is a mere extension of the sun's atmosphere. There are dynamical reasons which render this idea untenable; and consequently we are driven to the conclusion that the phenomenon is caused by the presence of a multitude of small bodies, moving in orbits of their own around the sun. In furtherance of this subject two points must be carefully borne in mind. The first is that there are phenomena of the light which indicate some resemblance, remote or otherwise, between its structure and that of comets' tails; so that not only meteoric matter, but also cometic matter, is probably present. The second is that it is highly improbable that the greater portion of the matter forming the light travels in orbits of small eccentricity around the sun. Knowing that the orbits of meteors extend far into space, sometimes beyond the orbits of Uranus and Neptune, we must suppose that the meteoric and cometic matter of its light would travel in paths similarly eccentric, so that it will at times be far beyond the bounds of its visible extent. According to this theory, the light should vary very markedly in appearance from time to time, and this, as a matter of fact, is precisely what has been observed, and remained unexplained until the eccentric nature of meteoric orbits was fully recognized. At the present moment the little bodies composing the light are believed to be something of the nature of Saturn's rings, the light, naturally, feebly reflected from the sun, which renders it so indistinctly visible. This we learn from the researches made by the polariscope and the spectroscope. When we speak of "little bodies" in a cosmical sense, we are naturally compelled to enlarge our ideas with reference to magnitude. Sir John Herschel remarks: "Compared with planets visible in our most powerful telescopes, rocks, and stony masses of great size and weight would be but as the impalpable dust which a sunbeam renders visible as a sheet of light when streaming through a narrow chink into a dark chamber." So it is with the Zodiacal Light. The sun goes down below the horizon, but the rays shoot up into the mass of solid particles which revolves around it, and illuminate their surfaces, the aggregate effect to an observer on the earth being that of a diffused sunbeam, taking the form into which the particles are grouped. Now this word "particle" must also be accepted in a magnified degree. Of course, it may happen that some of the constituent bodies are really small, and certainly none can be very large, otherwise they would not have escaped the careful scrutiny of the powerful, modern apparatus. We may believe, then, that the total mass must be almost nothing as compared to that of the sun. Hence, the central luminary can suffer no perturbation from the vicinity of these revolving bodies, though col-



Taken March 19th, 1901. Z 144. Aperture of lens 0.9 inch. Focal length 1.8 inch. Exposure 7 hours 52 minutes — 8 hours 0 minutes = 8 minutes.

* Reproduced from *Knowledge*.

lisions may occur among them, operating in the course of ages to effect a subsidence of at least some portion into the body of the sun, or perhaps into certain of the planets.

An unpractised observer frequently fails to notice the Zodiacal Light, when it is not difficult to perceive it, because the very gradual diminution of its brightness, both upward and laterally, deprives it of any definite outlines. We have the testimony of Schmidt, Jones, and other observers in support of the statement that it gradually fades away toward its edges. Jones especially calls attention to the variations in its brightness from time to time. Others have found it invisible, or at least very faint on some evenings, and it has been thought to be generally brighter in some years than in others. It is recorded that on January 31st, 1883, there were perceptible variations in its brightness relatively to the Milky Way. A distinction could be made between an inner and an outward zodiacal cone, mentioned by Jones as "the stronger and diffuse light." This might have been owing to the varying transparencies of the atmosphere through which the light is seen, or to varying sensitiveness of the observer's eye. With reference to the variation of brightness in different years, the evidence is not very conclusive. If one may venture to draw any inference from the various reports as a whole, it may be said slightly to favor the hypothesis of a variation in its brightness, coincident with the variation in the quantity of solar spots and of auroral displays, but the support at the best is not very strong. Among the few occasions when the light has been brighter than usual may be mentioned the month of March, 1843. That time was rendered remarkable by the sudden appearance of the great comet which caused then such a sensation. It so happened that the Zodiacal Light was noticed as brighter than usual; that Mr. J. Glaisher, writing from the Cambridge Observatory, made the singular mistake of confounding the two objects, saying: "The brilliant train which has for the last few nights attracted so much attention is doubtless only caused by the unusual brightness of the Zodiacal Light." The error was evident to all those observers who had seen the Zodiacal Light towering upward from the horizon, having the Pleiades near its vertex, while the comet came sweeping downward on the left. It is described as a magnificent spectacle not to be forgotten by any who witnessed it. It would seem as if the comet's presence were accompanied by some peculiar translucence of the atmosphere which rendered the Zodiacal Light more conspicuous.

There are also two phenomena which we may briefly notice in connection with this subject. One is the "Gegenschein," or "Counter Glow," and the other is the "Zodiacal Band." The "Gegenschein" is a faint patch of light seen very nearly opposite to the sun's place. Prof. Barnard has studied it for many years, and is of the opinion that it undergoes similar changes to the Zodiacal Light. He describes it generally as a large and elongated patch of light, but not visible in June and December, when it crosses the Milky Way. Not very much is known as to its structure.

The "Zodiacal Band" is simply a prolongation of the light of the "Gegenschein." On some occasions it has been seen to stretch across the sky at midnight from the "Gegenschein," and connecting the evening and morning Zodiacal Lights. It is about thirty to forty degrees in width, and is best seen when it passes between the Pleiades and Hyades. But both the "Gegenschein" and the "Band" are extremely delicate phenomena, and require not only a clear sky, but a clear sight to glimpse them.

It would be presumptuous to say that the phenomenon of the Zodiacal Light has been fully solved. Yet, notwithstanding the apparent faintness of its luminosity, the real amount of light must be considerable. It must not be forgotten that this luminous cone is reared in that part of the sky which is bathed in the strongest twilight. Could it be seen amid a darkened sky, instead of being almost in the arms of day, its brightness would make it so conspicuous as to attract universal attention, especially as it differs in form from every other celestial object. Unlike the sunbeams, as they shoot upward among the clouds during a gorgeous sunset, the Zodiacal Light is broadest at the horizon, and becomes narrower as it approaches the zenith. In the same way it differs from the fan-like rays of the Aurora Borealis. One thing, among the many, the heavens seem to tell with greater evidence from year to year is that space is not so void and empty as it was once thought to be, and that structures comparatively minute, as well as the immensely large, are widely spread throughout celestial space among the framework of the universe.

On the "Quantum" Theory of Light*

Is Energy, Like Matter, Atomic in Structure?

By L. P. Sieg, Ph.D.

Why are we interested in the subject of radiation? The answer is that radiation of so-called electromagnetic waves is the best connecting link we have between the two things that fill space: matter, and "not matter," or, let us say for the present, ether. If we can learn anything more about these two entities, then we are amply justified in our studies. Let me state then, at the outset, what information we have of these two entities, as connected by radiation. We know that every body in the universe is sending energy to every other body. We call the process radiation. You stand close to a stove and you are aware that something comes to your sense organs, which you call heat. You stand close to a cake of ice and you again detect its presence without the necessity of material contact. Your sophisticated mind tells you that heat is leaving your body more rapidly than it is being received. So, not to be diffuse in this regard, I state that we recognize that matter is radiating energy from itself, and is simultaneously receiving energy from outside. If we examine this radiant energy by suitable apparatus, we find that it seems to have all the properties of a wave, and further with other apparatus we can resolve this wave into component parts, running from waves of infinitesimally short lengths to those of infinite lengths. Not only that, but further, we find that the energy of this radiation is not equally distributed among these waves, but is so distributed that there is one maximum for a certain wave length, and that the energy falls off for wave lengths both shorter and longer than this one. It may be of interest to you to know that in an ordinary room the objects are all radiating, and that the wave length of this radiation that contains the maximum energy is about 1/3,000th of an inch, while on the other hand the sun's radiation finds its maximum at a wave length of about 1/50,000th of an inch, or in the yellow-green portion of the spectrum. The above illustration has anticipated my next point, which is merely that this radiation depends upon temperature and that the higher the temperature, the shorter the wave length at which the maximum occurs. This fact, coupled with the fact that the total energy sent out from a body is proportional to the fourth power of its temperature above absolute zero, constitutes a very essential part of our information of radiation from the standpoint of the hot body.

Now from the standpoint of the ether we have a very satisfactory body of knowledge. Maxwell long ago showed how an ether might be devised, possessing certain electric and magnetic properties, which would carry for us this complicated wave constituting radiation. When this radiation is once well started on its way in the ether, Maxwell's theory handles its progress beautifully. It accounts for a definite speed of propagation; it accounts for interference and diffraction. It accounts, albeit somewhat clumsily, for reflection at various kinds of surfaces, polarization and absorption. However, it does not connect the matter just discussed—temperature and radiant energy—with propagation through the ether. So after all, one can very seriously doubt, in view of what we know of radiation, that we know the connecting link between the heating of a body and the subsequent appearance in space of the radiant energy. There is missing a very important connecting link. This connection is missing, even in view of the modern electrical theories. To sum up this portion of the discussion, we say that in the radiation from material bodies there is associated with every wave length for a given temperature a certain amount of energy, which is a function solely of the wave length and the temperature. The exact type of this function we do not know. It has remained for the Quantum Theory to advance a type of function more nearly agreeing with the facts than any previously known. And, judged solely on this basis, it is a theory well worthy of our serious study.

Before we consider this recent theory, it is important to consider certain functions connecting energy with wave length and temperature, that have resulted from the study of classic thermodynamics and electrodynamics. Remember that any such function must satisfy a condition of equilibrium on the one hand between the kinetic heat energy of the molecules of matter and the electric contents of these molecules, and on the other hand between these electrical contents and what we are pleased to call free radiation in space. It will help greatly throughout this discussion if we imagine a certain space to be enclosed, with walls absolutely impermeable to radiation, and in this space to imagine present together matter in the form, say, of an ideal gas, at low pressure and at any given temperature, and its radiation. The walls are to be maintained at the same temperature as the enclosed gas. Further keep in mind that there is supposed to be equilibrium attained in this space, wherein a certain portion of the energy is in the matter, and a certain other portion is in the ether, and so the balance stays, no matter what constant interchange in this energy there may be.

We shall first look at Rayleigh's function which shall be discussed as briefly as possible. The energy possessed by the matter, if sufficient matter is present, is supposed to be divided up into as many portions as there are degrees of freedom, and each of these degrees of freedom possesses its even share of the energy. If there are N molecules present, there are $3N$ degrees of freedom, and so, too, $3N$ separate portions of the energy. Arguing on this basis, and using the laws of Maxwell, and laws from the kinetic theory of gases, Rayleigh arrived at a certain algebraic expression connecting the total energy and temperature. On the basis of the above laws, no one has successfully contradicted Rayleigh's formula. The only successful contradiction has come from experimental facts. The serious objections are these: His formula does not lead to the characteristic maximum of which I spoke at the beginning; the expression for the total energy is only true for long waves at any given temperature, and more seriously still, his hypothesis of equipartition of energy among all the degrees of freedom of motion will necessarily lead to an equilibrium state wherein all the energy in our enclosure is contained in the ether, while the matter therein is at practically absolute zero. This is because, from the evidence of continuous spectra, the ether possesses an infinite number of degrees of freedom.

It was in an attempt to so modify the idea of equipartition that it would not lead to inconsistencies, that led Jeans to announce a modification of Rayleigh's theory. In brief, Jeans admits the infinite number of degrees of freedom of the ether, but says that the ones characterized by extremely rapid vibrations, and those characterized by extremely slow vibrations are so vastly less important that the total energy is, so to speak, temporarily divided among the $3N$ degrees of freedom possessed by the gas molecules, plus the number of degrees in the ether between certain limits. The number of these degrees is by no means infinite. There is then the possibility of a quasi equilibrium between matter and ether, although in the course of a great time, the ether must take practically all the energy.

Yet another function has been developed, this one by Wien, from the standpoint of electrodynamics and the laws of thermodynamics. This function has been successful, in that it has accounted for a certain definite maximum of the energy at a given temperature and wave length. It is unsatisfactory because it fits the experimental facts only at the short wave-length end of the spectrum. It may be added that the exact function connecting these quantities has not been discovered. Wien simply has shown that as soon as the form of the function is known for one temperature, it becomes known for all temperatures. The laws of thermodynamics and of electrodynamics are not competent to give the exact form of the function. We must first form a clear picture of what is going on in the atom.

It remained for Planck to arrive at an expression for this function which seems to fit the facts of experiment with great exactness. It would be a physical impossibility for me to indicate in a short time the steps by which Planck arrived at his formula. His assumptions, however, are: first, that our matter in the enclosure is composed of a great number of vibrating resonators, each of which is the equivalent, if you please, of a wireless telegraph generator, i. e., a Hertz *oscillator*. Each of these has its own period. There is equilibrium between the vibrators and the ether, and the relation between these is obtained by electrodynamics. There is also equilibrium between the molecules and these resonators. It is in this last relation that Planck has made one of his unique assumptions. He has abandoned equi-partition of energy, and has arrived at the distribution of energy among the molecules and resonators through a consideration of probability or chance. It is strange that the same

* Abridged report of paper read before the Physics Section of the General Association of Science and Mathematics Teachers.

laws that govern legitimate games of chance should stand us in good stead for the solution of problems of radiation! I am sorry that I cannot take up with you, step by step, equation by equation, this remarkable development. Here, as in other portions of this paper, I must content myself with a few of the more interesting of the deductions.

Those who are familiar with physics will know that there are certain processes in nature that are irreversible. Among them are the natural expansion of a gas, conduction of heat from a hotter to a colder body, and heat lost through friction. There is a law in thermodynamics, the second law, that says of these three—that they are irreversible processes, it is the way Nature acts, and whenever she so acts, she has laid up against her records an increase in something we call entropy. Never has an absolute proof been given for the second law of thermodynamics. We have simply never been able to find an exception. Planck, from the work of Boltzmann, has found a reason for our lack of a proof of this law, for it cannot be proved. It rests on pure chance. In Planck's way of looking at it, the three facts spoken of above: the matter of expansion, conduction, and friction, should be stated in this fashion. It is very safe to say that these three things will always happen in nature, but it is not at all a certainty. In other words, a gas might sometime, of itself, contract; heat might sometime go, of itself, from a colder to a hotter body; and the heat usually wasted in friction might sometime turn itself back to useful work—just a matter of chance, although the chance is vastly in favor of the usual phenomena. Take the first case of the gas. The molecules are moving in all conceivable directions. Why might they not, at some time or other, take it into their heads, so to speak, to move all in one direction? If so, the gas could easily be seen to contract. So then Planck has used Boltzmann's expression for the entropy of our material body in terms of probability. But even this has not been sufficient. He has, lastly, in order to be able to count up the probability, been compelled to assume that the energy which a body expels, *must not flow out uniformly, but must be sent out explosively in integral numbers of a fundamental unit of energy.* He is able to account for uniform absorption of energy, but not for its uniform emission.

I have now, in brief, outlined the general aspects of the subject, and have shown, perhaps not nearly so completely and conclusively as might be desired, the points in which classic electrodynamics and thermodynamics have failed to account for even the most obvious facts of radiation. And lastly, I have outlined, in brief, the essential features of Planck's Quantum Hypothesis. We must now look into this latter theory and review those fields of radiation in which the theory has met not only success, but also failure, for you must know that the complete solution of the problem is by no means at hand. Again let me restate the situation, and enumerate the essential postulates of the theory, for it is on these postulates that we want to focus our attention throughout the remainder of this paper.

The general problem is to find a consistent connection between the energy of radiation in space and the heat energy in matter, this energy to be determined as a function of the wave length and the absolute temperature. Classic electrodynamics and thermodynamics, based as they are on Hamilton's Principle, which necessarily implies the validity of the concept of equi-partition among all the degrees of freedom, and based also upon laws of continuity, have been found to fail in the solution. Starting on quite new grounds and abandoning much of the classic principles of electrodynamics and thermodynamics, Planck has developed a wholly new function, and one, too, that has been eminently successful. The whole foundation of his theory is contained in his statement in the preface to his treatise on Radiation: "Entropy is a countable thing, varying from zero to infinity in its magnitude, and not, as in classic thermodynamics, running from positive to negative infinity." Seen more in detail, his postulates are three in number: first, equilibrium on the basis of electrodynamics between radiation and the oscillators in the atoms of matter; second, the entropy of a body is a function of the probability of its present state; and, lastly, the emission of energy from a body in finite units. I have already illustrated the bearing of probability on entropy. It will perhaps be well to look further into the reason for assuming definite energy units, and some of the consequences of this assumption.

The first, and most important reason for explosive radiation as distinguished from continuous emission of energy, comes from the necessity of being able to handle a finite number of energy units in determining the probability of any certain state of the body. Poincaré has said that since radiation depends upon vibrating matter, and since vibrating matter is discontinuous, then it is not at all illogical to assume that radiation is made up of discontinuities. Sir Oliver Lodge in his presidential address before the British Association warned his hearers that we may some day even come to the belief that time itself is doled out in discontinuous bundles, and that our existence may, in view of some future theory, prove to be no more continuous than the pictures thrown from the kinematograph. But to return to the discussion of these finite bundles of energy.

On both, sound theoretical and experimental grounds, Planck came to the conclusion that these energy units, while finite and definite, were not all of the same size. In fact, the expression for the magnitude of this element of energy is $\varepsilon = hn$, when n is the frequency of vibration of the oscillator, and h is a universal constant in radiation, called by Planck the universal *Wirkungsquantum*, or operating quantity. We must later say more of this important constant, h. We learn from this expression that when an oscillator has a high frequency, there is associated with it a large unit of energy, while a low frequency oscillator can emit a much smaller unit of energy. This gives us, without further details, an idea why so little energy is found in the violet end of the spectrum. Simply because in order to radiate here, it requires the absorption of much larger energy units. There are a host of verifications for this assumption in other realms of physics. For example, Nernst and his associates working on the subject of heat capacities of bodies at various temperatures, but particularly at extremely low temperatures, find remarkable agreement between experiment and theory. The reason for the low heat capacities at low temperatures and their increase with temperature, the reason for the law of Dulong and Petit concerning atomic heats, the derivation of many of the classic laws of gas theory—all have been indicators of the success of the Quantum idea, in this domain.

In light we find still more striking verifications. The photo-electric energy can be quite satisfactorily accounted for on the basis of this theory, as also can many of the characteristics of secondary cathode radiation. These two ideas I shall develop a little more fully in a later section of this paper. The apparently simple matter of line spectra has been a problem for the physicist, of no mean proportions. It is easy enough to say offhand that the lines in the spectrum of hydrogen, for example, are due to various electronic frequencies in the atom of hydrogen, when this atom is disturbed, but that is not at all satisfactory. You must know, of course, that an electron vibrating about the positive core of the atom, composes, with this core, a system in equilibrium. In this equilibrium state, with the electron vibrating regularly, no energy can be emitted, because by such an act, the stability of the system must be overturned. That means a change in the vibration frequency and if the emission of radiation is a continuous affair, the frequency change must be a continuous one, and there vanishes the possibility of lines in the spectrum. This past summer, Dr. Bohr, in the July and September numbers of the *Philosophical Magazine*, has proposed an explanation of the production of the hydrogen spectrum on the basis of the Quantum Theory, that is fascinating in the extreme. In brief, Dr. Bohr pictures the single electron in the case of hydrogen vibrating around the core in one of many stable elliptical orbits. No radiation takes place except where one of these stable orbits is changing to another one. Then the radiation is of one frequency, that corresponding to the frequency in the orbit nearer the center, and the energy change in the system is this same, $\varepsilon = h$ times the frequency. In this way, all the series of lines at present known in the hydrogen spectrum have been accounted for, and also some other lines have been predicted. Again in the subject of light we may account for fluorescence. By Stokes's law, we are told that the wave length emitted in fluorescent materials is lower in frequency than is the exciting wave. There are, however, some exceptions to this law. On the basis of the Quantum Theory the change in radiation means a change to a lower frequency, and hence to a lower energy. The energy lost has been absorbed, possibly by some slow moving, non-light emitting vibrator. In this latter case, we can say that the frequency is a function of the energy, rather than that the energy is a function of the frequency.

In dealing with absorption, Planck has, according to some physicists, taken a backward step, in that he has found it possible and also quite desirable to consider that the absorption of energy is continuous. One reason for this assumption is that the absorption of a finite amount of energy from an external steady radiation can take place only in a finite time. If the incident energy is too weak, none can be absorbed, and the smaller the intensity of the excited vibration is in comparison to an energy element ε, the longer the time for absorption will be. Now with increasing vibration frequency the energy element becomes very large, while we know from experience that the intensity of radiation for this large frequency becomes very small. Here, then, the time for absorption to take place would be far too great to agree with experiment. As a result of this especial hypothesis concerning absorption, it follows that a body which can only emit integral numbers of whole energy elements, and absorb partial elements, can be caught at absolute zero, still in possession of some energy. While at first thought this seems absurd, it is not so strange when we remember that we have defined absolute zero from the standpoint of molecular motion, and have not stated anything definite concerning the energy of the contents of the molecule.

All these experimental verifications from portions of physics outside those portions from, and for which Planck first outlined his theory, have been of material assistance in the spreading of his ideas. For it must be conceded that the Quantum Theory has needed these verifications, for the theory, as regards radiation phenomena, has not always followed the paths of correct logic. Further, the theory puts a great burden on the atom, for there must be present all these oscillators, each with its peculiar period. I have purposely avoided the use of electron, as a synonym for oscillator, just as Planck has done throughout, for the oscillator is at present only a name for something that may have many other peculiar properties, and until we know more of it, we had best use the original word.

Before starting the last section of this paper, I should like to say a few words in connection with the universal *Wirkungsquantum*, or Operating Quantity, h. If we have faith in Planck's theory, we must have the most wholesome respect for this quantity h. It occupies just as fundamental a position in energy as does the elemental charge e in electricity and matter. In fact, at the last meeting of the British Association, Jeans has found a strange and remarkable connection between e and h, two of the most fundamental things and probably the two fundamental things of physics. The relation is hardly definite enough for one to be called a function of the other, but the association of these two, even as tenuous as it seems, has in it possibilities that are fairly thrilling. The one primordial entity, the one universal regulator of the ceaseless ebb and flow of energy in our world of space and matter is being formed for us out of the very mists. Some have attempted to define h in terms of electrodynamics or mechanics, "But such an attempt," says Sommerfeld, "is as useless as a mechanical definition of Maxwell's equation." Energy does not depend upon molecular dimensions, constitution, and the like, but rather the very existence of molecules is a result of the existence of this universal operating quantity, h. Similarly in regard to energy. We have spoken of energy *quanta*, but these while fundamental are not all alike, so that energy itself cannot be conceived as being the fundamental thing in physics. Rather it is this same quantity h, which regulates and controls the manifestation of energy, that is to be looked upon as essentially basic. In his paper before the British Association this summer, Jeans declared for the new ideas, while still holding onto the old, by stating that Maxwell's equations should in their generalized form contain both the quantities e and h, the unit respectively of electric charge and the universal operating quantity. When these two quantities are missing from the equations, then we are dealing with special simplified cases. This is just the reverse of our usual views of this matter. Now, lastly, we have spoken so much of this quantity h, that you may have a desire to know if it has a numerical value expressible in terms of our fundamental units of length, mass and time. Its magnitude has been determined and proves to be the extremely small number, 6.415×10^{-27} g. cm ^{2}/sec., or to make it a little more striking, there is a decimal point followed by twenty-seven zeroes before the first significant figure. In this connection, I cannot refrain from mentioning a new set of fundamental units which Planck has suggested. To take the place of our units of length, mass, time and temperature, he suggests the use of this quantity h, the speed of light, the universal gravitation constant, and the value k in the formula for entropy, where the entropy is a constant, k times the probability of the state of the body. These four units depend upon the universal law of gravitation, the propagation of light in space, and the two laws of thermodynamics. On this system of units the absolute unit of length becomes the very short distance of 3.99×10^{-33} cm; the absolute unit of time and still smaller number, 1.33×10^{-43} sec.; the unit of mass, 5.37×10^{-5} g; and the unit of temperature, 3.60×10^{32} deg. Cent. Had I time, I could give you sufficient reasons for believing these units better than the ones we have. But we must go on to the last section, the one from which the title of this paper was taken.

Surely it does seem strange in this day for physics to ask again the world-old question—What is light?—but they are asking the question to-day with greater insistence than ever before. It did seem five to ten years ago as though we had a well-nigh perfect picture of the mechanism of radiant energy in space. However, the persistence of investigators in the domain of experimental physics has made it necessary, at the very least, for us all to review very carefully our ideas about this matter. The particular development of this subject into the nature of radiation in space, is not a development fathered by Planck, the founder of the Quantum Theory, who has been particularly conservative on this extension of the subject. We owe perhaps more to Einstein, the founder of the relativity theory, than to any other man. It must be stated that Einstein's bold ideas are accepted in their entirety by very few physicists; nevertheless, they are remarkable enough to warrant most serious study. The

whole question is, let me repeat: What constitutes light? Is it an electromagnetic wave passing out through the ether of space, or is it something quite different? According to the Quantum Theory of radiation, there is expelled from radiating matter energy in distinct bundles, indivisible for any given frequency. Now in what state does this energy go forth? We are confronted at the outset with a great difficulty in attempting to overthrow the theory of Maxwell, for we have abundant evidence from experiment that this theory does suffice. However, as I said above, there are other experiments according to which Maxwell's theory seems to fail completely. Surely we must not rest content with any theory that cannot explain everything that falls under its domain. On the other hand, we are not justified in abandoning a theory until we can get a more satisfactory substitute. First, a word concerning the much discussed ether.

I look on the word ether, as containing two meanings. One is the ether of the philosopher. If the philosopher is unhappy with space that is absolutely empty, then let him fill the space with whatever is necessary to his system of metaphysics. On the other hand, the physicist has an ether which is purely a child of his own intellect. The ether of the physicist is simplicity itself: It is merely a fabricated medium that has just sufficient properties to satisfy Maxwell's equations. If we try to give it any other properties we are getting into the philosopher's domain. If for any good and sufficient reasons we feel constrained to abandon Maxwell's wave equations, just that minute the ether of the physicist ceases to exist. The other man's ether may still be there, but it has no necessary function in the propagation of radiation. Let us follow in a very brief way some of the ideas that have led some physicists to abandon Maxwell's equations, and with these equations the ether of space.

The only thoroughly satisfactory theory of the state of the ether, and in speaking of the ether, I, for the remainder of this paper, shall refer to the physicist's ether, is that of Lorentz. He postulates a motionless ether. This has been tested by the famous Michelson and Morley experiment, with, however, only negative results. Arguing from this alone, and the steps of the argument are long and involved, Einstein comes to the conclusion on the principle of relativity that we come to absurd results unless we abandon the ether entirely. The absurdity comes from the conclusion, assuming Lorentz's ether and the principle of relativity, that we can imagine the ether at rest with respect to either one of two systems of co-ordinates moving through space with different velocities. This, of course, would be absurd. If we must abandon the ether, then the electromagnetic field constituting light is no longer a condition of a hypothetical medium, but represents rather separate images sent out by the source of light. Following this idea yet a little farther, Einstein shows quite conclusively that whenever a body emits radiant energy equal to L units, it loses thereby in mass by an amount L/c^2, where c is the velocity of light. This further strengthens his feeling that light is not a condition of a medium, but rather that it is something that has as independent an existence as has matter itself. But we need not go to such highly speculative sources. We have some common laboratory experiments that cause us sufficient food for thought. Why, for example, is the photoelectric potential independent of intensity? Let me illustrate in a manner, which, though crude, will perhaps be illuminating. Light a candle and place an insulated sheet of zinc, say one square centimeter in cross-section at a distance of one meter. The zinc attains practically instantly a certain positive charge, due to the liberation of the electrons from its surface. Repeat the experiment with the zinc plate held farther away. Again we get the same potential on the zinc plate, although the intensity of the light on the plate is now much less. If light goes out from the candle in spherical waves, surely the energy falling on a certain area becomes less and less, the farther away the plate is held. Campbell, in his recent book on "Modern Electrical Theory," has calculated that with a source of light of a given intensity a certain photoelectric potential should be acquired, on the basis of the electromagnetic theory only at the expiration of one quarter of an hour, because that time would be necessary, if the plate were perfectly absorbing, to accumulate enough energy to expel the electrons, and thus give the plate the potential that it gets in reality almost instantly. Now, if light is sent out by the candle in *quanta* or bundles, then no matter how far away the plate is, within reason, of course, it will be bombarded with these *quanta*. At any rate it will either show the full potential or none; it will not build up slowly. Again we have X-rays originating from the starting or the stopping of cathode rays. These X-rays fall on matter under proper circumstances, and cause thereby the emission of secondary cathode rays, which latter rays have energy of the same order of magnitude as those of the primary cathode rays. Surely this could not be if the waves constituting the X-rays should spread out uniformly in all directions, for that would lead to a contradiction of the law of conservation of energy. It certainly looks like a bombardment of compact bundles of energy, and not like the uniform spreading out of waves. If we could get a source of light feeble enough in energy it would emit explosively one or more *quanta* at a time in random directions, like the shooting of stars from a Roman candle in the hands of an extremely irresponsible person. For a bright light, multiply this millions of times. This surely is not the picture most of us form of the passing out of radiant energy.

Once more let me ask you to form a picture of the enclosed space containing impermeable walls and radiating matter. It is in connection with this special case that Einstein has exhibited what seems to me his most brilliant reasoning. Grant that the radiant energy is expelled from the matter in *quanta*, and assume for the once that the radiation is continuous. Imagine a small plane, perfectly reflecting surface inside the walls. This surface meets irregular blows from the molecules, supposed to be very sparse, and as a result of these irregular blows it will be moved, just as in the case of the Brownian movement. The radiation pressure on the other hand, is supposed to be perfectly continuous and equal on both sides of the plate, as long as the plate is stationary. However, when the plate moves, it must, in accordance with Doppler's Principle, experience a radiation friction which causes it to be retarded, but this in turn tends to convert the molecular energy into radiation energy. The result will evidently be that all the energy will ultimately become radiation energy, as in the case of Jeans's theory, while the matter will lose its heat energy entirely. The only escape from this is to assume that the radiation is as discontinuous as are the molecular impacts, and the average kinetic energy of vibration of the plate equals one third part of the kinetic energy of a monotomic gas molecule at this temperature. Continuing the argument, Einstein arrives at an expression for the momentum of the plate which consists of two terms: one the standard expression based on electrodynamics, and the other term based on Planck's energy *quantum*. The second term for the case when the temperature is 1,700 degrees and the wave length, that of sodium light, is 65,000,000 times as great as the electrodynamic expression; so much larger, in fact, that the latter can be neglected in comparison with the former. Lastly, quoting from Einstein: "If we should suppose that diffraction and interference phenomena were unknown, but that one knows the average value of the irregular variation in light pressure, where n would be a parameter of unknown meaning, determining color, then who would ever build up the wave theory?" However, we can think of the electromagnetic field as collected around singular points, just as in the electron theory, the electrostatic field is built up. When radiation is not sparse, the resultant condition of the field will be the same as on the wave theory, and there is no denying that the wave theory does work admirably for elementary discussion of light problems.

In closing this paper let me quote, this time from Planck: "The Quantum Theory is just as well founded, or even better so, than the old electrodynamics. Only some do not realize the limits of the latter. Years and tens of years are necessary, with much experiment. He who would to-day devote his efforts to the Quantum Theory, must, for a while, be satisfied with the consciousness that the full development of the present labor will not be reached until a later generation."

Heredity Versus Food in Development

It has become a dictum among those most interested in the raising of cattle that "breed rather than feed" determines the quality and output of milk. The food of an animal may be varied within very wide limits without altering the composition of its milk, provided that the ration is sufficient in amount. The only constituent which is known to be altered in milk by changes in the food-supply of the mother is the fat. The composition of butter may be affected somewhat by the food supplied to the cow; but normally little if any influence on the chemical make-up of the milk can be produced by wide variations in the mineral content of the food.

The question as to what extent, if any, the structure and development of the off-spring can be affected by the character of the maternal diet is somewhat analogous to that which concerns the possible alterations in the milk produced by the mother. An idea current among cattle-raisers is that a high mineral content in the ration will cause excessive bone formation in the off-spring.

It has been remarked that "assumptions in nutrition are dangerous." The questions raised in the foregoing discussion cannot well be tested by experiment on human beings; but the effect which a high lime-intake by the mother may have on the skeletal development of the off-spring has been subjected to experiment in the domestic animals. Since grains are deficient in calcium, farm rations made up wholly of them will not supply to growing animals a sufficient amount of this element. For this reason, growing or breeding swine fed entirely on grain, should receive an additional supply of calcium, either as calcium carbonate or calcium phosphate or in leguminous hay. Any drain on the organism with consequent loss of calcium is thus averted. The new Wisconsin experiments, however, have shown that though the nutrition of the mother has a great influence on the off-spring, the size is not modified by the liberal supply of any one element. Although a high calcium ration, containing over five times as much lime as the standard ration, was added to the feed of the mother during the entire period of gestation, no evidence was gained that the skeleton of the fetus was increased in any dimension or in calcium content thereby. If size can be influenced at all by the quantitative relation of the nutrients supplied, it is clear that many factors are involved and not a single mineral element. Size, says *The Journal of the American Medical Association*, is in a very large measure fixed by heredity.

Zirconium Hypophosphite a Photo-Sensitive Salt

By O. Hauser and H. Herzfeld

The hypophosphite is obtained by adding hypophosphorus acid to zirconium nitrate. The amorphous precipitate first formed redissolves in excess of the acid, and is precipitated by excess of alcohol as a colorless crystalline precipitate, which, when dried over calcium chloride, has the composition $Zr(H_2PO_2)_4$. In direct sunlight the hypophosphite becomes deep violet. Microscopic examination of the colored crystals shows no evidence of decomposition.—*Z. Anorg. Chem.*

SCIENTIFIC AMERICAN SUPPLEMENT

Founded 1876

NEW YORK, SATURDAY, JULY 18, 1914

Published weekly by Munn & Company, Incorporated
Charles Allen Munn, President; Frederick Converse Beach, Secretary; Orson D. Munn, Treasurer
all at 361 Broadway, New York

Entered at Post Office of New York, N. Y., as Second Class Matter

The Scientific American Publications

Scientific American Supplement (established 1876)	per year	$5.00
Scientific American (established 1845)	"	3.00
American Homes and Gardens	"	3.00

The combined subscription rates and rates to foreign countries including Canada, will be furnished upon application

Remit by postal or express money order, bank draft or check

Munn & Co., Inc., 361 Broadway, New York

The purpose of the Supplement is to publish the more important announcements of distinguished technologists, to digest significant articles that appear in European publications, and altogether to reflect the most advanced thought in science and industry throughout the world.

Table of Contents